With the author's compliments.

ANTISEPTIC SURGERY

ANTISEPTIC SURGERY

ITS PRINCIPLES, PRACTICE, HISTORY

AND RESULTS

BY

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TO

JOSEPH LISTER

TO WHOM ANY WORK ON ANTISEPTIC SURGERY

OWES ITS EXISTENCE

This Book is Dedicated

AS A MARK OF

THE AUTHOR'S ADMIRATION AND GRATITUDE

PREFACE.

The interest which Antiseptic Surgery has awakened throughout the profession has led to many public debates and to the accumulation of a great mass of literature on the subject. The time seems now to have come for a detailed account and discussion of the whole matter, and it is with the view of furnishing such an account that the present volume has been published. The question might have been dealt with in two ways; either by the discussion of the present standpoint of the principles and practice of Antiseptic Surgery, with but little reference to former literature; or, as I have attempted to do, by tracing in addition the development of its theory and practice, and following out and criticising the various steps by which the present state of knowledge on the subject has been reached. The former mode might no doubt have produced a shorter work, but it could not have been made complete in the present transitory state of our knowledge, and, taken alone, it would have necessarily raised side issues confusing to readers unacquainted with the earlier literature. I have therefore, in addition to the discussion of the present state of knowledge on the subject, tried to trace out its development, and I have done this the more readily because it seems to me that when an attempt is made, almost for the first time, to

discuss fully a new department of science, the work of those who have chiefly helped by experiment or criticism to build it up should be acknowledged and properly estimated. Such a way of treating the subject will perhaps also supply a want to those who have not the time nor the opportunity of searching out the literature for themselves. Among other facts which have been brought forward here I may draw special attention to the following, which I think have been now made certain:—Fermentations in wounds occur as the result of the entrance of particles—micro-organisms—from without; a variety of methods of treatment may be grouped under the heading 'Antiseptic treatment'; lives are saved in proportion to the asepticity of the wound, and, when the wound is kept aseptic, infective diseases more especially are avoided; the aseptic or Listerian method prevents the development of micro-organisms and the occurrence of fermentation in wounds.

Portions of this work have been previously published. Some investigations on micro-organisms in wounds and on temperature after operations formed part of my essay which gained the Syme Surgical Fellowship of the University of Edinburgh in 1877. I have since that time repeated and extended these investigations. In the essay to which was awarded the Boylston Medical Prize and the Boylston Gold Medal[1] of the Harvard

[1] By an order adopted in 1826 the Secretary of the Boylston Prize Committee was directed to publish annually the following votes:—

1st. That the Board do not consider themselves as approving the doctrines contained in any of the dissertations to which premiums may be adjudged.

2nd. That in case of publication of a successful dissertation, the author be considered bound to print the above vote in connection therewith.

University, United States, in 1880, I discussed the various methods of Antiseptic Surgery and the best modes of applying them to practice. And, in 1881, the council of the College of Surgeons of England awarded me the Jacksonian prize for a discussion of the history, principles, practice, and results of Antiseptic Surgery. This work is the outcome and development of these essays.

The drawings in this book (both the woodcuts and the plates) have been made by Mr. Edgar Thurston, to whom I am much indebted for the great pains which he has bestowed in their preparation. Mr. Groves also kindly took some photographs, from which certain of the woodcuts have been drawn. Dr. Heron has rendered me great assistance in revising the proof-sheets and in preparing the Index.

In dedicating this work to Mr. Lister I have attempted to acknowledge my great indebtedness to him.

W. WATSON CHEYNE.

6 Old Cavendish St., W.
October 1881.

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ANTISEPTIC SURGERY.

CHAPTER I.

THE PARTICULATE THEORY OF FERMENTATION. ON THE FERMENTATION OF BOILED SUBSTANCES.

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The term 'septic' so much used in surgery at the present day is derived from the Greek word *σηπτικὸς*, which means something that *causes* putrefaction, the verb *σήπω* signifying to *cause* to rot, to *make* putrid. An 'antiseptic' is therefore, according to this derivation, something which acts against the *causes* of putrefaction, and 'Antiseptic Surgery' is surgery directed not against its *effects* but against its *causes*. In dealing, then, with the subject of antiseptic surgery, we must first enquire what is putrefaction, and how is it brought about?

Putrefaction is now held to be a form of fermentation accompanied by the development of offensive odours, and fermentation may be defined as 'a new arrangement of the elements of an organic compound (often with the assimilation of the elements of water), and the consequent formation of new products.' (Fownes.)

Changes coming under the above definition of fermentation

have been long known as the result of what is termed Catalysis. Of this perhaps the best example is the change effected in Amygdalin by the action on it of Emulsin. As a result of the contact of these two bodies in presence of water, the amygdalin is broken up into various products, of which the chief are hydrocyanic acid and oil of bitter almonds. This decomposition of amygdalin is, however, not effected by *combination* with emulsin, for the latter remains unchanged, although its presence is necessary for the chemical action. Similar facts are known with regard to the Pepsin of the gastric juice, the Ptyalin of saliva, the pancreatic ferment, &c. But although these ferments undergo little or no change, yet nevertheless it has been clearly proved that they have not the power of self-multiplication. Hence these ferments generally receive the name of 'chemical' ferments.

There is, however, another class of ferments which possess this power of self-multiplication in a remarkable degree. An instance of this may be given in the alcoholic fermentation. Here a minute piece of yeast is introduced into grape juice, or into a sugary solution, and as a result fermentation soon sets in, and goes on slowly till the sugar is decomposed. If we compare this process with the former we see two marked points of difference; in the first place, in the former the change is rapid and more or less instantaneous; in the latter it progresses slowly and steadily, and requires much more time for its completion. In the former there is no increase in quantity of the ferment; in the latter the increase is very marked, and when the fermentation has gone on for some time the minutest portion of the fermenting substance added to unfermented material of like composition produces in it a similar series of changes; and this multiplication of the ferment goes on continuously whenever it is brought in contact with fresh material. As this power of multiplication is a property of living things, the term 'vital' is usually applied to this class of ferments.

In one point the 'chemical' ferment is allied to the 'vital' ferment. It is not a substance as yet formed by the chemist. It is the product of living cells; and it is quite possible that the yeast plant may act in the same way as the salivary or

peptic cells, viz. by excreting a ferment which produces the change in the fermentescible substance, this ferment in neither case possessing the power of self-multiplication. In the case of the salivary ferment the cells which produce it are an integral part of a complex organism, and cannot live apart from the body; hence the ptyalin, introduced into starch, does not increase in amount. On the other hand, the yeast plant is an independent cell, and grows free in the fermenting fluid, and it is to the growth and multiplication of these living cells, and not to an increase in quantity of the chemical ferment as such, that the multiplication of the fermenting power is due.

In the putrefaction of discharges in wounds we have to deal with a ferment belonging to the same class as the alcoholic ferments—with a ferment capable of multiplication, which acts slowly and steadily, not suddenly. We must therefore enquire a little more fully into the nature of this class of ferments, into their origin and history, in order to obtain some principles to guide us in attempting to prevent their action.

In reviewing the history of this subject, the first research of any consequence which it is necessary for us to consider is that of Gay-Lussac.[1] Previous to the publication of this paper attempts had been made by Lavoisier, Fabroni, and Thenard to give some explanation of the process of fermentation.

Lavoisier's work was in the main carried out with the view of ascertaining the changes which a fermenting liquid undergoes.[2] He did not attempt to assign a definite cause for the process.

Fabroni,[3] writing on the subject of the alcoholic fermentation, concluded that fermentation was a decomposition of one substance by another, 'just as a carbonate is decomposed by an acid, or sugar by nitric acid. The substance which decomposes sugar is a vegeto-animal substance. It is contained in certain utricles in the grape. When the grape is crushed this material, which is of the nature of gluten, mixes with the sugar in the juice, and as soon as these two substances come into contact, effervescence or fermentation commences just as occurs in every

[1] *Annales de Chimie*, lxxvi. 1810.
[2] *Éléments de Chimie*, i. 2nd ed.
[3] *Annales de Chimie*, xxxi. 1799.

other chemical process, as, for instance, when an acid and a carbonate are mixed in the same vessel.'

Some years later, but ignorant of Fabroni's views, Thenard published a research on alcoholic fermentation,[1] in which he showed that gluten had no power whatever of causing the fermentation of sugar. He observed that during the process of fermentation, a deposit occurred which had the power of inducing a similar change in a fresh saccharine liquid. This substance was apparently the same in a great variety of liquids of different chemical composition, and it presented characters similar to those of yeast. Thenard states that he was unable to determine whether this substance was formed in the course of the fermentation, or whether it was in solution at the commencement and became deposited as a result of the changes which occurred. He, however, inclines towards the latter view.

Gay-Lussac was led to make his investigations by studying the procedure of M. Appert for preserving vegetable and animal substances.[2] Appert's method consisted in placing the materials to be preserved in bottles, very closely corked. These bottles were exposed to the temperature of boiling water for a longer or shorter period of time. They were then packed up and kept for use. There can be no doubt as to the efficiency of this method, for in Appert's work certificates are furnished by several scientific commissions, containing such names as Gay-Lussac, Bordel, &c.

Gay-Lussac noticed that, though the substances so prepared could be preserved unaltered for an indefinite period so long as the vessels were kept thoroughly closed, yet, as soon as the vessels were opened, and more especially if the substances were decanted into other vessels, their contents underwent fermentative changes.

To ascertain why this occurred, he took a flask of grape-juice which had been preserved for a year unaltered, and which was accordingly quite limpid. Having opened the flask he poured its contents into another vessel, which he closed very accurately and kept at the temperature of 15° to 30° C. Eight days later

[1] *Annales de Chimie*, xlvi. 1803.

[2] *The Art of Preserving Animal and Vegetable Substances.*

the juice had lost its transparency, fermentation had become established, and it soon became an alcoholic fluid. A second vessel containing grape-juice, prepared by Appert's method, was kept at the same temperature and in the same place, but unopened. This remained pure. This latter flask, the neck of which had been drawn out and sealed, was now taken, and a deep notch having been made with a file, its neck was plunged into mercury and then broken off. A portion of the contents was then introduced into a bell-jar containing no oxygen, and a second portion into one containing a small quantity of that gas. The first remained without change for forty days, while the second underwent fermentation very rapidly. In the latter flask all the oxygen had disappeared, but much more carbonic acid in proportion had been produced. Gay-Lussac therefore concluded that, although oxygen is necessary to initiate the fermentation, yet it is not essential for its continuance. The same results were obtained when currant-juice or freshly prepared grape-juice were used. Gay-Lussac further found that, if this juice after being transferred from one vessel to another were again heated after secure corking, it could again be preserved for an indefinite time. He observed that during the boiling the fluid lost its transparency and a deposit took place. He came to similar conclusions as to the necessity of oxygen for the initiation of fermentative processes, in the case of the putrefaction of meats, &c., preserved according to Appert's method.

Gay-Lussac concludes from his experiments that oxygen is necessary for the commencement of the fermentation; that this oxygen combines with some substance in the fermentescible fluid, thus producing the ferment, which can then act without further oxygen; that the effect of the heat in Appert's method is to decompose any combination already formed, and to make the oxygen which is present unite to form some substance which is not a ferment; that the product of this union with oxygen is the deposit which is seen to occur on heating these fluids. He however recognises that fermentation is still a mysterious process, since it occurs slowly and not immediately like other chemical actions.

Passing now over a period of some years, we come to the

views of Caignard-Latour, made known in papers presented to the French Academy during the years 1835–37.[1] On examining fermenting grape-juice, he found (as indeed had been imperfectly observed before by Leuwenhoeck and Desmazières) that it contained numerous globular bodies which he considered to be of vegetable nature, and which he found to possess the power of reproduction, partly by budding and partly, as he supposed, by contracting and liberating numerous spores. From several facts—amongst others, from finding that in juices not undergoing alcoholic fermentation these bodies were absent, while they were always present where that fermentation occurred—he concluded that they were the cause of the fermentation. He further found that the deposit of which Thenard spoke, and which he had stated to be the ferment, was composed entirely of these bodies.

Similar views were announced almost simultaneously by Schwann,[2] and to him rather than to Caignard-Latour must be given the credit of having furnished the first real proof of the view that these cells were the causes of the fermentation.

Schwann prepared infusions of meat, fruits, &c., somewhat after Appert's method, but, instead of leaving the vessels completely sealed, he allowed air which had been previously heated to come in contact with the fluids. The following is his description of the method which he ultimately adopted.

'Into a three-ounce vessel a small piece of meat was introduced, and then water was added so that the whole occupied about one-fourth of the capacity of the vessel; the bottle was then closely corked, the cork being firmly fastened down by wire.' This cork was traversed by two small glass tubes, one of which was at once bent downwards on its exit from the vessel, and its orifice dipped into a small beaker containing mercury covered by a layer of oil. The other tube ran at first horizontally and then directly downwards for an inch and a half. There it had two narrow spiral turnings, then it again ran upwards, and finally horizontally, being drawn out to a fine point at its termination. The cork was covered with several layers of a solution of caoutchouc in linseed oil, rendered thinner by the addition of oil of turpentine. The fluid in the flask was now boiled, and the steam was made to issue from the two tubes till the mercury and oil became so

[1] See *Annales de Chimie et de Physique*, t. lxviii. 2e série, 1838.
[2] *Poggendorf's Annalen*, xli. 1837.

hot that they no longer condensed the steam. (In order that no organisms might develop in the water which remained between the oil and the mercury, a layer of corrosive sublimate was placed between them.) While the boiling was going on a spirit lamp was placed under the spirals of the second tube, and the heat was continued until the tube began to soften (see Fig. 1). The drops of water condensing in the cooler parts of the tubes were dispelled by another lamp. After boiling had continued for a quarter of an hour it was stopped, and, during the cooling of the flask, air passed through the second glass tube into the vessel, being however previously heated in the spiral part of that tube. After complete cooling of the flask the orifice of this tube was sealed and the portion of the tube between the spiral and the end, containing unheated air, was heated. That being done the spirit lamp was completely removed.'

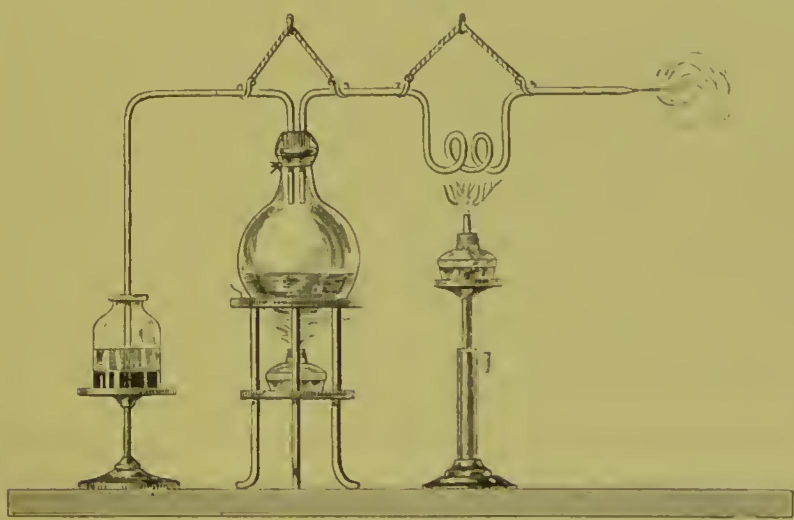

FIG. 1.—SCHWANN'S METHOD.

Prepared in this way, the flask contained only boiled meat infusion and heated air. From time to time this air was renewed in the following manner: the spiral part of the tube having been heated almost to melting, the point was broken and fresh air forced slowly in, the old air bubbling out through the mercury. After a time the tube was again sealed with the same precautions as before.

By operating thus Schwann succeeded in preserving meat and other substances at a temperature of 63° to 77° F. without any putrefaction, and without the appearance of organisms in them; while the same materials when exposed to ordinary air underwent putrefactive changes in a few days.

It was thus evident that there was a something present in the air, other than the gases of the air which had the power of bringing about fermentative changes in boiled liquids, and that this something could be destroyed by heat.

Some further experiments which Schwann performed with reference to the alcoholic fermentation furnish additional evidence against the view that the gases of the air are the causes of putrefaction.

A solution of cane sugar having been mixed with yeast, four flasks were quite filled with the mixture and then corked. These flasks were placed in boiling water for an equal length of time (ten minutes each). They were then inverted over heated mercury, and air was introduced so as to displace one-third to one-fourth of the fluid. The flasks were afterwards corked under mercury and kept at a temperature of 63° to 77° F. In two flasks the air thus introduced had been previously heated, in the other two it had not been so treated. In four to six weeks the flasks which had received the unheated air burst, their contents having undergone alcoholic fermentation. The other two flasks remained unchanged for more than three months.

Sehwann states that this latter experiment with heated air is generally, but not always, successful, and he explains this by the supposition that after the heating of the mercury, and in the proeesses of uncorking and eorking, organie matter, which had not been previously heated, might very possibly mix with the mereury and enter the flasks. (This view has since been proved to be eorrect by Pasteur.)

It is but fair to refer here to the experiments on spontaneous generation published in 1836 by Franz Schulze. These will be deseribed at a later period. They are, however, of interest here, beeause Schulze antieipated Schwann in the principle of admitting air previously aeted on in some way or other, in his ease ehemically, to Appert's preserves.

Schwann's experiments were repeated and eonfirmed by Ure in 1840[1] and by Helmholtz in 1843.[2] The latter author, in order to test the oxygen view still further, prepared an infusion in the usual manner in a vessel into which platinum electrodes were fixed. After the infusion had remained unaltered for some

[1] *Journal für praktische Chemie*, xix.
[2] *Müller's Archiv*, 1843.

time a current of electricity was sent through the liquid, decomposing the water into hydrogen and oxygen. But even this oxygen in its nascent, and therefore in its most active state, was unable to produce any fermentation in the fluid.

In the meantime Liebig had stepped forward as the opponent of the views advanced by Caignard-Latour and by Schwann. He proposed a theory of a totally different nature.[1] After pointing out that organic molecules have a tendency to alter their constitution, to break up and rearrange themselves, he approaches the question of putrefaction and fermentation. With regard to the cause of these changes he writes as follows: 'Cette cause est la faculté que possède un corps en décomposition ou en combinaison, c'est-à-dire, en action chimique, d'éveiller la même action dans un autre corps qui se trouve en contact avec lui, ou de le rendre apte à subir l'altération qu'il éprouve lui-même.' He compares this sort of action to combustion, and cites several well-known instances of this kind, such as the decomposition of nitric acid by an alloy of platinum and silver, a change which does not take place with the platinum alone; or the decomposition of peroxide of manganese by oxide of silver, &c., in water containing oxygen.

He includes under the term 'eremacausis,' the alterations which organic substances undergo at ordinary temperatures, and as the result of the action of oxygen. When oxygen is excluded, putrefaction occurs. Putrefaction is a combustion of one or more of the elements of the organic substances at the expense of their own oxygen. Where no foul smelling products result, the process is termed fermentation. Putrefaction occurs when the equilibrium of the attractions of a complex organic molecule is upset, and it results in a rearrangement of these elements. Non-nitrogenous organic substances do not undergo this putrefaction spontaneously when they are pure. They must be brought in contact with some substance already undergoing change. This latter substance is therefore termed a ferment. This ferment is a nitrogenous substance undergoing putrefaction and eremacausis, it converts the oxygen of the air into carbonic acid, &c. Its activity is destroyed by desiccation, by heat, alcohol, &c. It is the soluble portion of

[1] *Annales de Chimie et de Physique*, 2e série, lxxi. 1839.

the ferment which is active, and this does not act by direct contact, but in consequence of a decomposition which it itself undergoes. The elements of the ferment take no part in the formation of the products which sugar furnishes when fermented, though at the same time the ferment is itself undergoing destruction. (This latter statement is based on an observation by Thenard, who found that 20 parts of fresh yeast, added to 100 parts of sugar left, after fermentation was complete, only 13·7 parts of an insoluble residue. This residue placed in a new portion of sugar became reduced to 10 parts. This last residue no longer exerted any action. Pasteur has since completely disproved the accuracy of Thenard's results. On the contrary he finds that yeast increases markedly during fermentation, and in his latest work Liebig admits this.)

The ferment is, therefore, according to Liebig, a body undergoing decomposition. If the ferment is too small in quantity for the sugar, when the decomposition of the former is complete the latter ceases to ferment (this statement has since been shown to be quite erroneous), and therefore a sufficient quantity must be present in order that its decomposition may not be completed till that of the sugar has ended. No special substance is, according to this view, required in order to act as a ferment, but merely one which shall be the constant exciter of action in the fermentescible substance. (This statement is also wrong, because putrefying fluids and tissues added to sugary solutions do not convert the sugar into alcohol. This was latterly admitted by Liebig, who was compelled to allow the existence of some relation between the yeast plant and the alcoholic fermentation, a connection which he, however, attributed to the effect propagated from the dead and dying, not from the living, cells.)

Liebig sums up as follows:—

Organic compounds present two opposite and definite phenomena.

1. They give rise to bodies endowed with new properties, the elements of several molecules of a more simple compound uniting to form one molecule of a more complex nature.

2. Some complex molecules of a high degree of complexity

break up into one or more less complex molecules of a lower order, in consequence of the destruction of the equilibrium of the attractions of their elements. This equilibrium may be destroyed by heat, by contact with a body of different composition, or by the action of a body which is itself undergoing change.

As an example of this Liebig takes the case of urine: 'In fresh urine,' he says, 'if oxygen be entirely excluded there occurs no alteration of the urea or of the hippuric acid contained in it, but if exposed to the air another substance present in the urine (probably the mucus) undergoes a change of form and composition (eremacausis), which is transferred or communicated to the urea and the hippuric acid; the urea is resolved into carbonic acid and ammonia; the hippuric acid disappears, and in its place is found benzoic acid.'

He continues: 'When we reflect that the power of exciting putrefaction belongs to bodies of the most different composition, that blood, flesh, cheese, saliva, infusion of malt, emulsion of almonds, &c., acquire this property as soon as, by the chemical action of oxygen, a disturbance in the equilibrium of the attraction of their elements has taken place, all doubt as to the true cause of these phenomena seems to disappear.' (We shall see later that in this Liebig is wrong, that meat, cheese, &c., cause putrefaction not from any inherent tendency to do so or from any state of decay, but because they introduce the necessary particles into the putrescible liquid.)

Liebig then goes on to say, with reference to the germ theory of putrefaction, that after the death of fungi and infusoria we observe the same putrefactive phenomena as after the death of a larger animal. These organisms, according to him, only appear at a late period of putrefaction, and therefore are not the cause of it, though no doubt by their vital actions they must hasten and modify the change.

And now let us pause in the history of this important subject, and methodise somewhat the views expressed. These may be divided into three sets. Firstly, we have that of Gay-Lussac, who attributes putrefactive and fermentative changes solely to the influence of oxygen in the first instance.

Then come the views of Caignard-Latour and Schwann, referring these changes to the entrance into the fluids of solid particles from without, which may be destroyed by heat. These authors go further, and ascribe the whole fermentative process to the growth of the organisms which are found in the fermenting liquids.

And, lastly, we have the view of Liebig, who looks on oxygen as in so far favouring fermentation that it causes eremacausis, the molecules undergoing this change being now capable of setting up putrefactive and other fermentative changes. The latter changes are therefore due to the presence of some substance itself undergoing change, and to this substance the term 'ferment' is applied. This ferment may be destroyed by heat.

It will be more convenient if for the present we class these views under two heads—the oxygen theory (Gay-Lussac's) on the one hand, and the particulate theory (Schwann's) on the other. At a later period we shall determine whether Liebig's or Schwann's is the more tenable view.

I have already mentioned the researches of Schulze, Schwann, Ure, and Helmholtz as tending more or less to upset the views of Gay-Lussac.

The next research of importance on this subject is that by Schroeder and Dusch.[1] Their aim was to see whether *filtration* of the air would be sufficient to prevent the fermentation of boiled fluids. Their apparatus was the following:—

A glass vessel containing the material to be tested (meat infusion, &c.,) was closed by a close-fitting cork, which was dipped into hot wax previous to its insertion. This cork had two holes in it which gave exit to two tubes bent outside to a right angle, these tubes being also firmly embedded in the cork; one tube was for the purpose of conducting air to the vessel, and the other to suck air out of it.

The conducting tube was connected by means of a short piece of vulcanised caoutchouc with a glass tube; the latter was again attached to a wider tube (1 inch in diameter and 20 inches long) by means of a similar cork to that in the bottle, and at the other end of this tube was a cork with a bit of tubing in it, called the open tube. The wide

[1] *Annalen der Chemie und Pharmacie*, 1854.

tube was loosely filled with cotton wool which had been previously heated for some time in a water bath.

The other tube—the suction tube—which in the interior of the flask reached almost to the level of the fluid, was connected by means of a vulcanised india-rubber tube with the upper tube of an ordinary gasometer, this latter tube being provided with a stop-cock. The gasometer was filled with water, and on opening the lower tube of this vessel the water flowed out and thus the gasometer acted as an aspirator. (See Fig. 2.)

Such was the apparatus employed. The fluid having been introduced into the flask, and all the connections having been

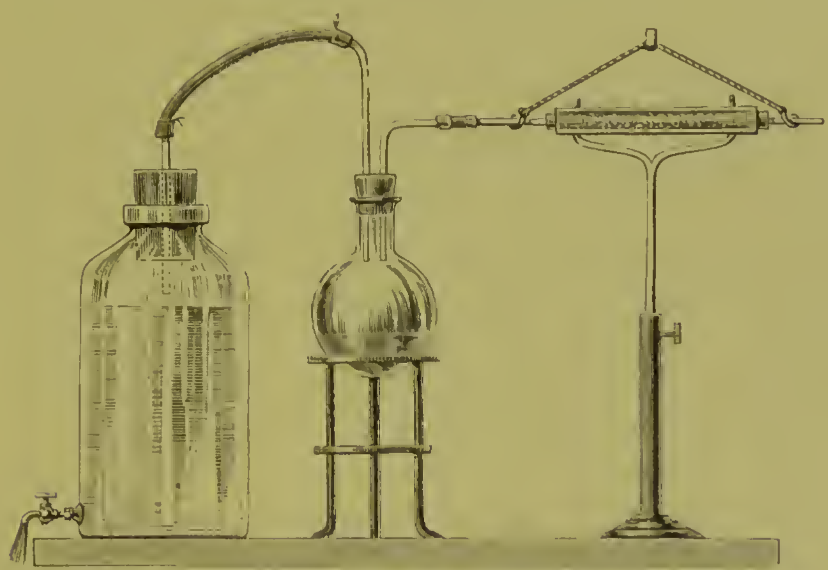

FIG. 2.—SCHROEDER AND DUSCH'S APPARATUS FOR SUPPLYING FILTERED AIR.

(The tube to the left ought to reach almost to the level of the liquid.)

ascertained to be air-tight, the cock of the aspirator was shut and the substance to be tested was boiled, till all the tubes as far as the cotton wool had been thoroughly heated; then, the joinings having been again examined, the aspirator was so arranged that water flowed out of it drop by drop, and thus sucked air slowly through the flask.

Meat, boiled in water and kept in a flask of this kind with constant change of air, was preserved for twenty-three days, and when tested at the end of that time was found to be quite unaltered, while a similar infusion left exposed to ordinary air had to be removed from the laboratory during the second week on

account of its intolerable stench. This experiment was repeated several times with similar results.

Schroeder and Dusch experimented in a similar manner with fresh sweet malt containing hops. After twenty-three days, the fluid being still unaltered, the cotton wool was removed, but the introduction of air—now unfiltered—was continued. The fluid was muddy and covered with fungi, and had undergone fermentation in a week.

These observers were, however, unable to obtain like results with milk, or with meat heated without the addition of water. These substances invariably underwent putrefaction.

In a paper published five years later Schroeder returns to this subject.[1] Having found that white of egg mixed with water, if constantly shaken while boiling, could be preserved for an indefinite time, he tried whether *ozone* had any power in inducing fermentation. Dilute sulphuric acid was decomposed by electricity, and the ozone thus generated was conducted into a vessel containing pure white of egg. The latter was kept for thirty-eight days, and was at the end of that time unaltered.

Schroeder was still unable to succeed with milk or yolk of egg, although the latter, if previously heated in a closed vessel in an oil bath to 160° C. (310° F.) generally remained unchanged, and the milk sometimes also kept pure.

The special constituents of milk could be easily preserved. He tested casein prepared by precipitation with acetic acid and then washing with water. The whey also which remained after this precipitation could be preserved with ease. When this whey was boiled a deposit occurred, and this was readily kept pure; and the whey which still remained did not ferment when preserved with the precautions mentioned.

Schroeder tried and succeeded with other materials, such as blood, urine, starch, &c.

The only substances which failed were milk, yolk of egg, meat heated without addition of water, and occasionally meat infusion.

As the result of these contradictory experiments he came to the conclusion that there were two ways in which fermentation might be caused; firstly, by some solid particle which

[1] *Annalen der Chemie und Pharmacie*, cix. 1859.

can be arrested by cotton wool, and, secondly, by oxygen gas (in the case of milk, yolk of egg, &c.).

Two years later there appeared another paper by Schroeder referring to those substances which he had previously failed to preserve, and in this research he has recourse to the use of higher temperatures than formerly.[1]

Yolk of egg, after being heated for half an hour in a closed glass vessel, at a temperature of 130° C. (266° F.), was placed in a flask the neck of which was stuffed when hot with cotton wool, and was again boiled with a little water. This remained for seventy days unchanged.

He succeeded in a similar manner with meat and milk, and in the case of the latter he found that prolonged boiling at 100° C. was sufficient.

From these facts he gives up his formerly expressed view as to the spontaneous fermentation of organic substances under the influence of oxygen, and concludes that in these fluids spores were present which could resist a boiling temperature, the development of these spores being, according to him, the cause of the fermentation. He further considers that these spores were present originally in the milk, and were not introduced from the air, because he finds that milk which has not been boiled at all putrefies sooner than pure boiled milk exposed to the air.

It may be interesting to mention here that similar difficulties were experienced by Appert in his attempts to preserve milk. He succeeded by the following method: 'Condense the milk to two-thirds of its volume, strain it, then put it in the bottle, seal and boil in a water bath for two hours.' In order to prevent the cream from separating he found it well to add yolk of egg. This did not increase the difficulty in preserving it.

Still further evidence disproving the gaseous theory is furnished by Pasteur.[2] He repeated Schwann's experiments and was successful with most fluids, but for a time he failed in the case of milk. He, however, succeeded when he boiled the milk under pressure at 110° C. (230° F.) for one or two minutes, heated air being then allowed to come in contact with it; and

[1] *Annalen der Chemie und Pharmacie*, cxvii. 1861.

[2] *Annales des Sciences Naturelles*, série iv. t. xvi. 1861: *Zoologie*

he also succeeded if he subjected the milk to prolonged boiling at 100° C. Such milk remains unaltered for an indefinite length of time, but it readily decomposes if unheated dust be introduced into it in the manner to be afterwards described.

The most striking of Pasteur's experiments is that of the flask with the bent neck. A flask containing, say, yeast water is heated so as to render its contents pure. Its neck is drawn out and bent, and then, after boiling, the lamp is simply with-

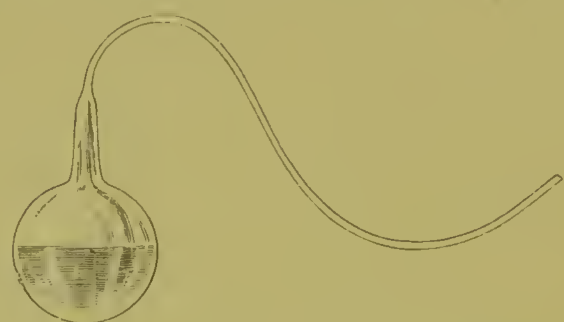

FIG. 3.—PASTEUR'S FLASK WITH THE BENT NECK.
(From Pasteur.)

drawn, the neck being neither heated, sealed, nor plugged (Fig. 3). Nevertheless the fluid does not undergo any change.

If, on the other hand, the neck of this flask be sealed during ebullition, a more or less perfect vacuum is thus produced, and then, if the neck be broken after cooling, air rushes violently into the flask, carrying with it its dust. The result is that fermentative changes occur in the fluid. In the same way, if one of the flasks with open necks, the contents of which have remained for some time pure, has the neck broken off short, the fluid in its interior rapidly undergoes fermentation; or again, if the neck be not bent but be kept straight, so as to allow dust to fall in, fermentation rapidly occurs.

The explanation of these results is that in the case of the flask with the long neck the dust is caught in the curve, which in the first inrush of air is filled with water, which filters the air (Mr. Lister's view). Pasteur had supposed that part of the air dust entered the vessel instantly, but that the fluid and the walls of the flask were at that time so hot that any living particles present were immediately destroyed.

As will be seen further on, Pasteur also found that it was

not necessary to filter the air of its dust, but that if the air were merely left undisturbed for some time till the dust settled, it might then be introduced into flasks without causing any development.

These experiments have been repeated by various observers with success, and Mr. Lister has at present in his possession a flask of this kind containing urine which is now thirteen years old, but which still remains unaltered and as limpid as on the day it was prepared.

In a lecture on Haze and Dust,[1] Professor Tyndall showed that if no dust were present in a flask, a beam of condensed light passed through the vessel in a dark room would only be visible on each side of it, but would be invisible in its interior; in other words, we see light only because there are particles in the air which render it visible. Were there no particles all would be darkness.

Such being the case, Tyndall found that when the air which was admitted to a flask had been previously heated, as in Schwann's experiment, the beam of light was not visible in the interior, showing that all or almost all the particles had been destroyed by heat, or, in other words, were in the main of an organic nature. By the same method Mr. Lister has found that in Pasteur's flasks with the long open necks, no floating dust is present after what was originally there has settled.

Another method of excluding dust was published in 1873 by Mr. Lister.[2] It seemed probable that the occasional failures which occurred in the attempts to preserve boiled fluids arose from the fact that the steam did not destroy the septic energy of the dust in the necks of the flasks which had not been previously heated. Mr. Lister, therefore, in addition to the precautions as to boiling under cotton-wool caps, &c., subjected his flasks to a high temperature previous to the introduction of the fluid. This is done by keeping them, after the cotton cap has been applied, in an iron box, which is kept at a high temperature for two hours.

This box is of a square form, with one of its sides movable so as

[1] *Nature*, Jan. 27, 1870.

[2] *Microscopical Journal* for October 1873; see also *Trans. of Path. Society of London*, vol. xxvii. 1878.

to form a door. 'This door has its circumferential part in the form of a groove capable of being packed with a considerable mass of cotton wool (Fig. 4, F). This door can be secured by means of nuts against the

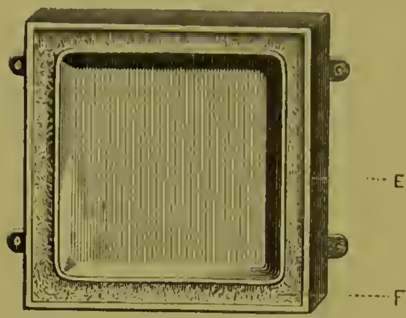

FIG. 4.—DOOR OF MR. LISTER'S BOX.

edge of the box; and the cotton wool, having the narrow rim of metal thus firmly pressed against it, serves as an effectual filter of the air that passes in during cooling. But then it is essential that the heat be so equally distributed as to avoid heating any portion of the cotton to such a degree as to destroy its physical properties. This uniformity of heat is provided for by having three shelves of sheet iron interposed between the large Bunsen's burner and the bottom of the box, so as to prevent the heat from acting directly upon it, while at the

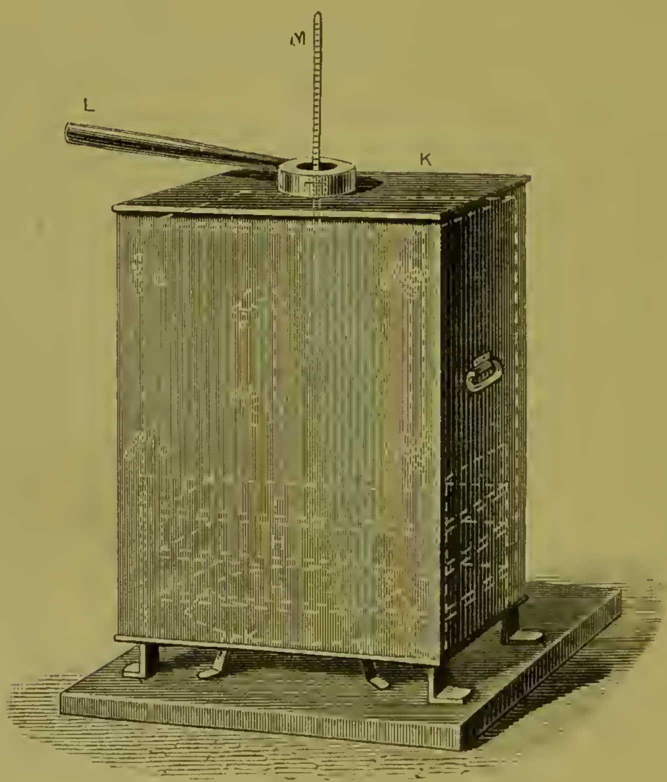

FIG. 5.—MR. LISTER'S HOT BOX.

same time the box is covered over with a cover of sheet iron (Fig. 5, K), which reaches nearly to the ground, and, while it checks radiation, compels the heated air to travel over the whole exterior of the box

and escape by holes at the top of the cover, whence it is conducted into a chimney by a tube (L). By these two means combined, the shelves below and the cover round about, we get a uniform browning of the cotton. Into such a box the requisite number of vessels are introduced (Fig. 5). An aperture in the top of the box well packed with cotton wool transmits a thermometer (M), to show us when the temperature of 300° F. has been reached, and when this, or any other higher degree short of 350° F., has been continued for two hours, the gas is turned off and cooling is allowed to take place; and when the apparatus is quite cool, the covered glasses may be removed with confidence that they are perfectly free from living organisms.'

In this manner Mr. Lister purifies his flasks. The larger flasks have two necks, a large vertical one and a lateral one, which is a bent spout, large at its commencement and comparatively narrow at its shorter terminal part beyond the bend (Fig. 6, O). The large size of the first part prevents it from acting as a siphon, and the result is that when the liquid is poured from such a flask and the vessel is afterwards restored to the erect position, the end of the nozzle remains filled with a drop of the liquid, and this guards the orifice so that regurgitation of air can never take place through the nozzle. This drop of fluid being sucked away by means of a carbolised rag, a pure cotton cap is tied over the orifice, and the flask is kept for future use.

FIG. 6.—MR. LISTER'S LARGE DOUBLE-NECKED FLASKS.

This flask, purified by heat and with each orifice covered with pure cotton caps, is used for the experiments (see Fig. 7). The fluid to be tested is introduced into it by means of a siphon, consisting of two glass tubes (S and T) connected by a tube of india-rubber (U), with a stop-cock (V) in the course of the india-rubber tubing. The siphon is first completely filled with water, the temperature of which should be higher than that of the air, so that there is no dissolved air given off to form bubbles. Place one leg of the siphon in the vessel containing the fluid to be used (W), then turn the tap and permit a sufficient amount of fluid to flow out to ensure that all the water has escaped from the siphon; then turn off the stop-cock, wash the outside of

the tube (T) with carbolic lotion, wrap a mass of carbolised rag (Y) around its lower extremity, and apply this to the mouth of the flask (X) as soon as the cotton cap is removed; push the tube steadily down to the bottom of the flask through the carbolised rag, turn the stop-cock, and let the required amount of fluid flow into the flask (Fig. 7). When this has taken place the tap is again turned off, the

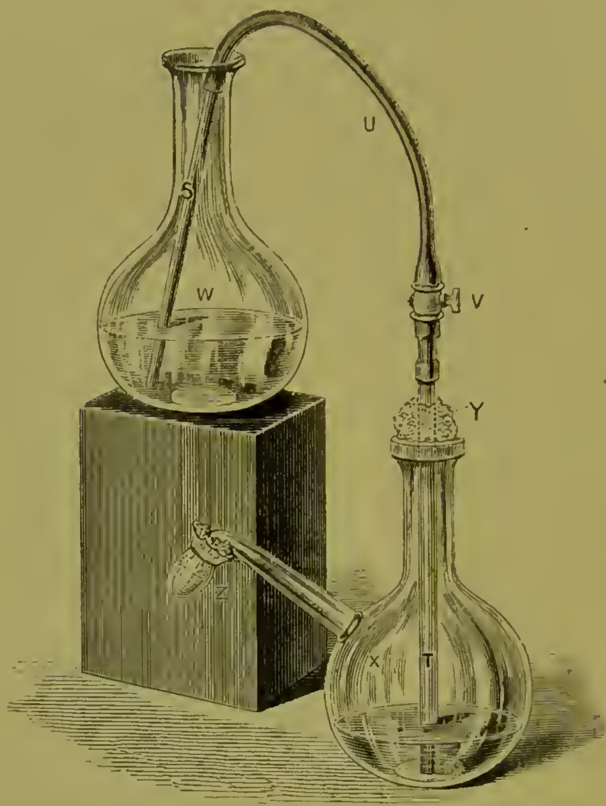

FIG. 7.—METHOD OF FILLING THE FLASKS.

siphon is withdrawn through the antiseptic rag, and a fresh cap of carbolised cotton (the cotton is carbolised by being soaked in a solution of one part of crystallised carbolic acid in one hundred parts of anhydrous ether and allowed to dry) is tied over the mouth of the flask when the rag is withdrawn. The fluid is now heated for the desired length of time, and then abandoned under the protection of the caps.

In this way Mr. Lister has found that he can preserve turnip infusion, hay infusion, urine, fresh milk, &c., for any length of time without any alteration taking place. To preserve milk, the flask containing it is immersed in boiling water for half an hour or more.

But this is not all, for these fluids can be transferred to smaller vessels without undergoing any fermentation after this transferrence. This is done as follows: a liqueur glass (A) is covered by a glass cap (B) (watch glass), and the whole by a glass shade (C), the liqueur glass and the shade standing on a glass plate (see Fig. 8). This arrangement is introduced into the hot box and thoroughly purified. Thus we have a pure glass filled with pure air, and the problem is to transfer the fluid from the flask to the glass without contamination in the process. To do this, the cotton cap is removed from the nozzle of the flask (Fig. 7, Z) and the end of this is instantly slipped into an opening in the centre of half an india-rubber ball (Fig. 9, R) previously steeped in a strong watery solution of carbolic acid. The outer glass shade is then removed, and the watch glass being lifted, the india-rubber cap is instantly applied in its place (see Fig. 9). The required quantity of fluid is then poured into the glass, and the cap and shade immediately reapplied. A fresh cotton cap is now tied over the nozzle of the flask. In this manner any number of glasses may be charged, and these are found to remain as pure and unaltered as the fluid in the original flask.

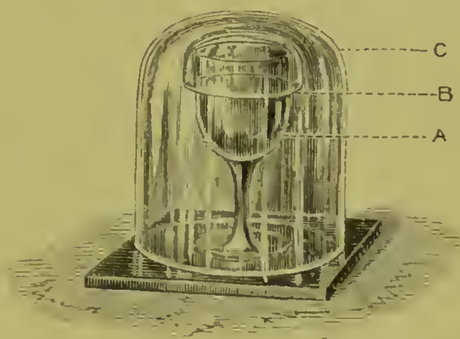

FIG. 8.

FIG. 9.

And now observe what such experiments teach. In the first place, into the original flask, when cooling, air enters, but this air having passed through a cotton-wool plug is incapable of causing putrefaction. Then in the decanting of this liquid from the flask, fresh air must enter through the large mouth of the flask, but as this passes through a filter of cotton wool it is in like manner incapable of causing fermentation. Further the liqueur glasses are full of air, which has either been previously heated, or which has been filtered

through the cotton wool around the door of the hot box. The fluid when poured from the flask into the glass mixes freely with this air, but no change is set up. And, lastly, the loosely-fitting glass cap and shade allow a free interchange of air, but are so placed as to make sure that the air deposits its dust outside the glass, thus corresponding in action to Pasteur's flasks with the bent necks. In spite of all these opportunities of admixture with the gases of the air, all sorts of fluids remain unaltered, while, on the other hand, the same liquids exposed freely to unfiltered air rapidly undergo fermentative changes.

These experiments are of themselves an absolute proof that the gases of the air alone are unable to cause fermentative changes in organic substances.

In 1874 Dr. Roberts[1] demonstrated again that fresh milk and other substances could be prevented from putrefying if kept in a flask with a cotton-wool plug after having been previously boiled.

His method was as follows: An ordinary delivery pipette, having on it an oblong bulb capable of containing 30–50 cc., was sealed at one end, and the materials to be experimented on were then introduced into the bulb until it was two-thirds full (Fig. 10, A). The inside of the neck of the bulb was next wiped dry, and a plug of cotton wool was inserted about its middle. Lastly, the neck was drawn out above the plug and sealed in the flame (Fig. 10, B).

When the bulb was thus charged and sealed it was weighted with a leaden collar, and submerged in the upright position (so as to prevent the wetting of the cotton-wool plug) in a can full of water. The can was placed over a source of heat and boiled for the required time. The bulb was then withdrawn and, when quite cold, its neck was filed off above the cotton-wool plug (Fig. 10, C). Finally it was set aside in the upright position and maintained at a suitable temperature.

By exposure to the heat of boiling water for from twenty to forty minutes Roberts was able to preserve those substances with which Schroeder and other observers had failed, viz., milk, pieces of meat, and egg albumin.

In 1876 experiments were published by Professor Tyndall[2]

[1] *Philosophical Transactions*, 1874.

[2] *Philosophical Transactions*, 1876.

which afford still further evidence on this subject. I have already mentioned the method by which he demonstrated the presence or absence of particles in suspension by passing a powerful beam of light through the air to be examined. He found that 'in order to render air optically pure it was only necessary to leave it to itself for a sufficient time in a closed chamber or in a suitably-closed vessel. The floating matter gradually attaches itself to the top and sides, or sinks to the bottom, leaving behind it air possessing no scattering power. Sent through such air the most concentrated beam fails to render its track visible.'

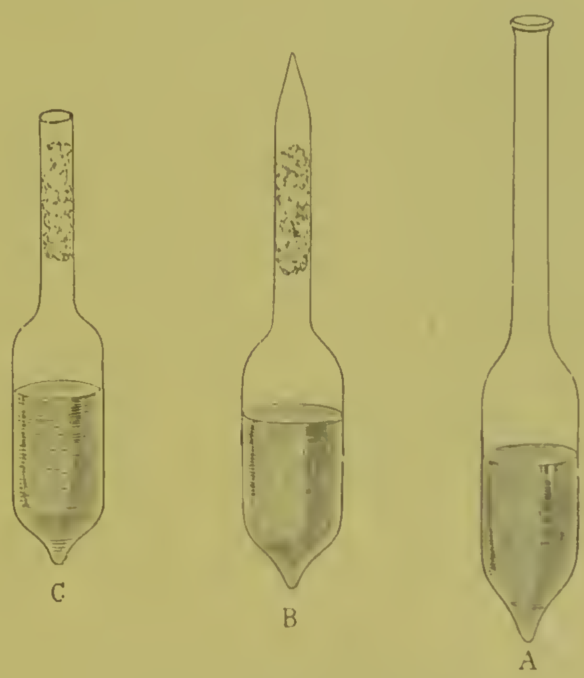

FIG. 10.—DR. ROBERTS' BULBS (COPIED FROM ROBERTS).

His method as described by himself is as follows: 'A chamber or case was constructed with a glass front, its top, bottom, back and sides being of wood. At the back is a little door which opens and closes on hinges, while into the sides are inserted two panes of glass facing each other. The walls of this case are smeared with glycerine in order to make the dust adhere. The top is perforated in the middle by a hole 2 inches in diameter, closed air-tight by a sheet of india-rubber. This sheet is pierced in the middle by a pin, and through the pinhole is passed the shank of a long pipette ending above in a small funnel. A circular tin collar 2 inches deep surrounds the pipette, the space between both being packed with cotton wool mois-

tened with glycerine. Thus the pipette, in moving up and down, is not only firmly clasped by the india-rubber, but it also passes through a stuffing box of sticky cotton wool. The width of the aperture closed by the india-rubber secures the free lateral play of the lower end of the pipette. Into two other small apertures in the top of the cupboard are inserted, air-tight, the open ends of two narrow tubes intended to connect the interior space with the atmosphere. The tubes are bent several times up and down so as to intercept and retain the particles carried by such feeble currents as changes of temperature might cause to set in between the inner and the outer air (see Fig. 11).

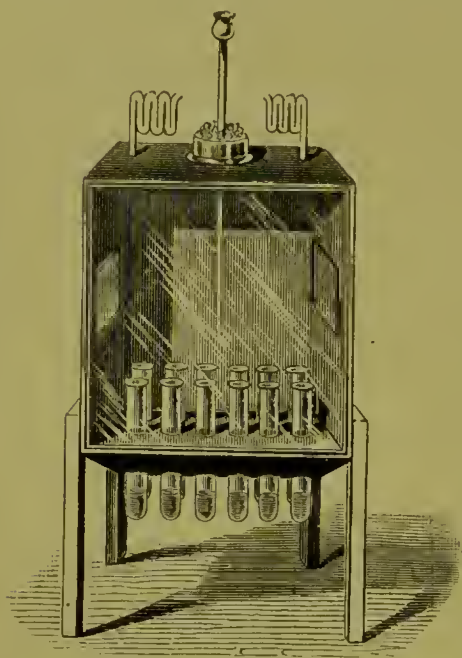

FIG. 11.—PROF. TYNDALL'S PURE CHAMBER (COPIED FROM TYNDALL).

'The bottom of the box is pierced with holes, in which are fixed, air-tight, twelve test tubes, intended to contain the liquid to be exposed to the action of the moteless air.'

The case so prepared is closed and allowed to stand for three or four days, till it is found by the beam of light that all the dust has settled. Then, the pipette being dipped into the test tubes, the fluid to be experimented on is introduced into each in succession. They are then boiled for five minutes in a brine bath. During the cooling, plugs of cotton wool are introduced into the small external convoluted tubes, but these plugs are afterwards withdrawn. The apparatus is then kept at a suitable temperature and at perfect rest. At the same time a part of the same infusion boiled for the same length of time is placed outside the box in free contact with the air.

In this way Tyndall has been able to preserve for an indefinite time, boiled urine, mutton infusion, beef infusion, haddock infusion, turnip infusion, hay infusion, infusion of sole, liver infusion, infusion of hare, rabbit, pheasant, grouse, codfish, turbot, herring, mullet, fowl and kidney; while flasks containing the same infusions, left exposed to the air after boiling for the same length of time, invariably putrefied in a few days.

This experiment, though resembling in many respects Pasteur's experiment with the flasks with long bent necks, differs from it materially. In Pasteur's experiment the whole of the interior of the vessel is acted on by the heat, and thus when the boiling is ended there is no part of the flask, except the neck, which contains any particles capable of causing fermentation. In this case, however, the steam from the tubes, passing into a larger chamber, is not able to destroy the vitality of the dust lining the walls of that chamber, and therefore the infusion is here not only in contact with ordinary air which has not been acted on by heat nor filtered of its dust, but the septic dust itself is present in the same vessel though not in actual contact with the fluids. Tyndall found that as soon as ordinary laboratory air, laden with dust, was admitted, putrefaction commenced.

Tyndall has further shown that the gases arising from putrefying substances, however foul smelling, cannot produce decomposition in other fermentescible liquids, although this readily occurs when ordinary dust is admitted.

Thus, 'on the 30th of November a quantity of animal refuse, embracing beef, fish, rabbit, hare, was placed in two large test tubes, opening into a protecting chamber containing six tubes. On December 13th, when the refuse was in a state of noisome putrefaction, infusion of whiting, turnip, beef, and mutton were placed in the other four tubes; they were then boiled and abandoned to the action of the foul sewer gases emitted by their two putrid companions. On December 25th these tubes were still unchanged. On the same day the end of the pipette was dipped into one of the putrid tubes and then inserted into the turnip, and on the 27th a similar speck was transferred to the whiting. These rapidly underwent decomposition, while the remaining two tubes remained unaltered.'

By operating in the manner described by Mr. Lister I have succeeded equally well in preserving fresh milk, meat, cucumber or turnip infusion for any length of time. As I shall have to refer at a later period to experiments in which extensive use is made of the ease with which these fluids can be preserved though retaining great readiness to undergo fermentation, I need not say more at present.

Not only is air which has been filtered incapable of causing fermentation in a boiled liquid, but air which has been

acted on by carbolic acid is also without effect. I may mention a few facts made out by myself in support of this statement.

In the small room in which most of my experiments were done it was almost impossible for me to transfer fluids from one flask to another, by Mr. Lister's method, without contamination and subsequent fermentation, but if I performed the same manipulations in a spray of about 1 to 30 carbolic acid and water I could transfer all sorts of fluids with ease from one flask to another without any risk, even though done in the most leisurely manner. In doing this I have used Mr. Lister's double-necked flasks without the protection of the india-rubber cap. I have also in a few instances simply poured the fluid from one single-necked flask to another, and when this was done in a carbolic acid spray without other precaution, the fluid remained pure.

That milk once rendered barren by boiling can be readily preserved for any length of time, though retaining its capability for undergoing fermentation, is shown by the following experiment.

On January 30th milk was prepared by boiling for twenty minutes in a flask purified by boiling distilled water in it under a cotton cap, the flask being afterwards dried by heat.

On the same afternoon three purified tubes with glass caps and shades (just like Mr. Lister's liqueur glasses) were half filled with this milk under the spray.

February 6.—The caps were removed under the spray, and a heated needle being introduced, portions of the fluid were taken from each tube for microscopical examination. All the milks were found to present the normal appearance of fresh milk externally and microscopically.

February 11.—Examined as before. No change.

February 19.—No change.

March 3.—Still fluid and unchanged in appearance. Two of the tubes were now tested by the addition of a drop of fluid from a tube containing milk which had been left open, and which had putrefied. In three days the milk in these two flasks had separated into two layers, and had lost its normal characters.

April 11.—The milk in the third test tube still remains unchanged.

I might multiply instances to show that milk and other fermentescible fluids can be kept in this way for months at a

suitable temperature, without undergoing any change. This is not due to any effect of the carbolic acid on the milk, because milk so preserved rapidly undergoes fermentation when exposed to the air. Indeed the minute quantity of the solution which comes in contact with it can have no effect whatever, as is shown by the following experiment performed in 1877.

February 1.—Five pure test tubes were taken and into each was introduced 100 minims of boiled milk, along with a certain number of minims of watery solution of carbolic acid, 1–20.

To No.	I. were added	2 mins.,	making a proportion of			1–1000.
,,	II. ,,	5	,,	,,	,,	1–400.
,,	III. ,,	10	,,	,,	,,	1–200.
,,	IV. ,,	20	,,	,,	,,	1–120.
,,	V. ,,	50	,,	,,	,,	1–60.

They were then shaken up and left exposed to the air for twenty-four hours, and afterwards covered with very loosely-fitting caps, which were removed at intervals during the following day.

February 6.—The milks were beginning to alter in appearance and to separate into layers. This was the case even in No. V.

April 19.—They were all much advanced in decomposition.

Thus we see that decanting can be safely done in a spray of carbolic acid, the fluid still remaining as putrescible as ever; while, on the other hand, experience had taught me that in the particular room to which I have referred, it was very difficult to decant successfully without the spray.

The following experiment which I performed some time ago directly proves the efficacy of the spray :—

Two flasks containing pure milk were opened in my room, and left open for ten minutes. In both bacteria developed. As soon as these flasks were removed two other flasks similarly charged were put in the same place in a fine cloud of carbolic spray. They were opened and left open for ten minutes. Both of these remained pure, though when inoculated at a later period organisms rapidly developed in them. When they were removed the spray was stopped, and two fresh flasks were placed in the same position, opened and left open for ten minutes. One of the latter remained pure; in the other organisms appeared. (As will be later seen, the presence of organisms is synonymous with the presence of fermentation, and their absence with the absence of such changes.)

Another experiment proves to demonstration the efficacy of the spray in destroying the putrefactive agents in the air :—

Four flasks provided with cotton caps were purified according to Mr. Lister's method. Into two of these, pure cucumber infusion was introduced in the manner already described. These two flasks were placed for four days in an incubator, kept at the temperature of 98° F. At the end of this time the fluid was unchanged in both. About half of the liquid in one of the flasks was then poured into one of the empty previously purified flasks, in a cloud of carbolic spray, and the caps reapplied. These were then placed in the incubator and they remained permanently unchanged, and without the development of organisms. The same process was gone through with the two flasks without the use of a spray. In both of these organisms developed and putrefactive changes occurred.

In this experiment, when the fluid was poured from one vessel to the other it passed through the air, and air also entered into the first flask to take the place of the liquid. When this air had not been acted on by carbolic acid, organisms developed and fermentation took place, but where the air had previously passed through the spray it failed to cause any further change. (I do not of course mean to imply that the former will be a constant result, for in ordinary air there are but few organisms present, and probably many flasks would escape. This experiment refers to the air of the room in which it was performed, that air being loaded with causes of fermentation.)

A very striking proof of the value of the carbolic acid spray which occurred to me lately may be mentioned. The flasks which I at that time used were purified by heating them to a temperature of about 600° F. in a box like that described by Mr. Lister. The flasks were in the first instance heated without any covering, the cotton caps were then applied under the spray, and the flask with its cap reintroduced into the box, where it was thoroughly dried in order to drive off any carbolic acid which might be adhering to it. As the temperature to which the apparatus was in the first instance raised chars cotton wool, I used asbestos to filter the air as it passed into the interior of the box during cooling. For a while this answered quite well, but after a time portions of the asbestos became detached, and holes were thus formed through which air could

enter without being filtered, and as a result on several occasions I found that all the flasks so prepared were impure. This was obviated simply by directing the spray against the door of the box as soon as the lamp which heats it was extinguished. The box was thus surrounded by spray; the air passing into it first passed through this spray, and, as a result, since I did this, I never failed in any instance in obtaining pure flasks.[1]

From these researches we learn that the gases of the air, whether oxygen, nascent oxygen, ozone, nitrogen, carbonic acid, emanations from fermenting substances, &c., are powerless to cause fermentation in boiled fluids or tissues. Further, that it is sufficient, in order to prevent this occurrence, that the air be either previously heated, or filtered through cotton wool, or acted on by chemical substances, such as sulphuric acid or carbolic acid, or merely allowed to remain at rest so as to permit the dust to settle outside the substance tested. It is therefore evident that the causes of putrefaction in *boiled* substances are solid particles present in the atmosphere and on surrounding objects, which may be deprived of their fermentative properties in various ways. When we come to consider the further questions of spontaneous generation and the relation of organisms to fermentative changes, we shall find much additional evidence confirming this view.

[1] The exact merits of the carbolic spray as a means of purifying the atmosphere will be discussed later. What I wish to point out here is, that in ordinary air, in circumstances where we know that particles capable of causing fermentation are present, carbolic acid is able to render these particles inert.

CHAPTER II.

THE PARTICULATE THEORY OF FERMENTATION (*continued*).—ON THE FERMENTATION OF UNBOILED SUBSTANCES.

Grape juice—*Gay-Lussac—Van der Broeck—Pasteur—Roberts*; blood—*Van der Broeck—Pasteur—Burdon-Sanderson—Lister—My own results*; unboiled urine—*Van der Broeck—Pasteur—Lister—My own results—Roberts—Cazeneuve and Livon*; milk—*Hoppe-Seyler—Roberts—Lister—My own experiments*; egg albumen—*Van der Broeck—Gayon—Roberts—My own experiments*; vegetable tissues—*Roberts*; animal tissues—*Billroth—Tiegel—Burdon-Sanderson—My own experiments—Chiene and Ewart—Meissner*—General review of the facts—Behaviour of similar fluids and tissues in the living body—Principles of aseptic surgery.

While it cannot be doubted that the causes of the fermentation of *boiled fluids and tissues* are particles which reach them from the air and from surrounding objects, is this equally the case with the *unboiled*? Experiments with these substances are apt to yield very contradictory results, for it is a matter of extreme difficulty to prevent their contamination after their removal from the living body. How this has been managed and with what results we must now enquire.

I.—*Grape Juice.*

Gay-Lussac in the research mentioned before attempted to ascertain whether unboiled grape juice remained unaltered when oxygen gas was excluded. He took a bell jar and introduced into it small grapes, still intact. The jar was now reversed over mercury, and was filled five times with hydrogen gas in order to wash out all the oxygen. The grapes were then crushed by means of an instrument introduced through the mercury, and the juice thus obtained was kept at a temperature of 15° to 20° C. Fifteen days later, no fermentation having taken place, a

small quantity of oxygen was introduced, and immediately fermentation commenced.

From these experiments he concludes that the oxygen introduced caused the fermentation. But here there are two main fallacies. In the first place, the skins of the grape were left mixed with the grape juice, no sufficient means being taken to destroy any solid particles adhering to them; and then also the oxygen introduced might have carried in the necessary particles. There can indeed be no doubt, from Pasteur's subsequent investigations, that the Torula cerevisiæ—the cause of the alcoholic fermentation—was present on the skins of the grapes; and Pasteur has further shown that oxygen is absolutely necessary for the development of the old cells of the Torula, though the young cells may go on developing without the presence of free oxygen. The explanation of Gay-Lussac's experiment is, therefore, that the old Torula cells present could not develop without oxygen, but that when a small quantity of oxygen was introduced, they developed, and fermentation occurred.

The next attempt to preserve grape juice of which I can find any record was made by Van der Broeck, and narrated to the 'Provincial Gesellschaft für Kunst und Wissenschaft,' Utrecht, January 1858. His method was the following[1]:—

Small beakers were filled with mercury, and then heated in a sand bath till the boiling point of the mercury was almost reached. From time to time they were placed under the receiver of an air-pump, and at the same time shaken in order to detach any bubbles of gas adhering to the side of the flask. This process of heating and exhausting was continued till all the air was removed from the bottom or sides of the glass. These glasses were then inverted in a basin containing previously heated mercury, and were firmly fixed in this position. Ripe and uninjured grapes were now passed into the mercury and brought under the orifice of the flask, a portion of the skin of the grape was clipped out by a heated knife, and by gentle pressure some of the juice was made to ascend in the vessel, the rest of the grape being removed. When a sufficient quantity of juice had been thus introduced the vessels were placed in a room of which the temperature was 25° to 28° C., and grape juice thus obtained could be kept for months or years without undergoing any change.

[1] See *Annalen der Chemie und Pharmacie*, cxv. 1860.

In this experiment not only was all air excluded, but the dust adhering to the walls of the vessel and in the mercury was subjected to strong heat, and its fermentative power destroyed. The juice of the grape, in ascending through the mercury, did not come in contact with unheated dust, nor did it touch the skin of the grape.

Into some of the flasks containing pure grape juice obtained in this way, pure and fresh oxygen was introduced from a retort containing chlorate of potash and oxide of copper. (The nozzle of the flask was heated previously to its immersion in the mercury, and the oxygen was allowed to stream out for a time sufficient to wash out all the dust.) In none of these flasks was there a trace of fermentation. Into others, atmospheric air, passed through a mass of cotton wool, was introduced in the same manner, but without producing any effect.

Into these vessels containing oxygen, yeast was introduced in minute quantity, and fermentation at once commenced. Into others containing only grape juice, young cells which had never been exposed to free oxygen were introduced by a method which is fully described in his research, and these also caused fermentation; thus proving that oxygen is not necessary even for the commencement of the change, if only the yeast cells be young (three or four days old).

By these experiments it was absolutely demonstrated—1 That oxygen is not the cause of the fermentation of unboiled grape juice; and 2. That the juice itself contains no ferment.

That grape juice contains no ferment was further shown by Pasteur,[1] who introduced unboiled juice into his flasks with bent necks, containing pure boiled juice. No fermentation occurred, though, as he says, if a single Torula cell had been added, the whole mass would have fermented.

Dr. Roberts [2] likewise succeeded with grape juice.

Test tubes were drawn out at their lower ends to capillary points and sealed in the flame; the upper ends were plugged with cotton wool; they were then passed and repassed through the flame of a spirit lamp until they were quite hot, as shown by the commencing charring of the cotton. (Fig. 12.)

'Eleven sterilised tubes, six empty and five containing water, were

[1] *Etudes sur la Bière.* [2] *Phil. Transactions*, 1874.

charged with grape juice in the following manner:—A fresh grape was firmly seized with the finger and thumb, and a spot on its surface was pressed for a few seconds against the flame of a spirit lamp so as to destroy any adhering germs. The point of the sterilised tube, also heated in the flame and quickly snipped off by an assistant, was then thrust into the grape at the heated spot. Compression was now made on the grape until a sufficient quantity of the turbid juice was forced into the tube. The tube was then withdrawn, and its point sealed in the flame. The eleven tubes thus charged remained permanently unchanged, and when examined, at various periods from five to eight weeks, the taste and reaction of their contents were undistinguishable from that of the fresh grape juice.'

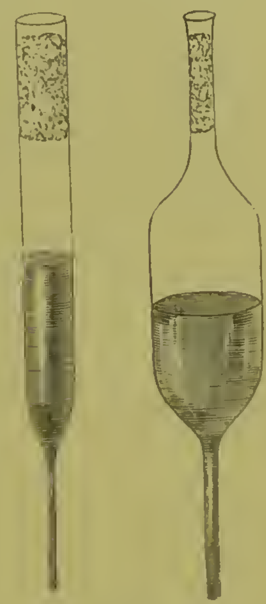

FIG. 12 (FROM ROBERTS).

II.—*Blood.*

Blood is one of the substances which have been frequently referred to as having an inherent tendency to decompose, but several experiments have now demonstrated that this is not the case.

The first observer who succeeded in preserving blood was Van der Broeck.

Van der Broeck proceeded as follows:—Having prepared his beakers filled with mercury as formerly described, he introduced one end of a previously heated copper tube into the carotid artery of a dog. To the other end of this a caoutchouc tube was connected, while the free end of the latter dipped into the mercury and the blood passed along it into the purified beakers. (This caoutchouc tube had been purified by the passage of steam through it for some time, and by placing a plug of cotton wool in each end while it was cooling.) The vessels were then kept at a temperature of 25° to 30° C. for weeks without the contained blood undergoing any change.

Into some of these flasks oxygen or filtered air was introduced, but still there was no putrefaction. The minutest portion of putrescent or even non-putrescent but unheated substance at once set up fermentation.

In 1863, Pasteur [1] stated that he had obtained blood from

[1] *Comptes Rendus*, lvi. 738.

healthy animals by means preventing contamination with unheated atmospheric dust, and that this blood had remained free from change. In a later publication [1] he describes the method pursued.

'For this I made use of a flask connected by means of a caoutchouc tube with a brass tube and stop-cock. The two parts of the tube are about twelve centimetres in length; that which is free is filed down like the extremity of a canula. In order to cleanse this vessel from all living dust the free extremity of the brass tube was connected with a platinum tube strongly heated, a small

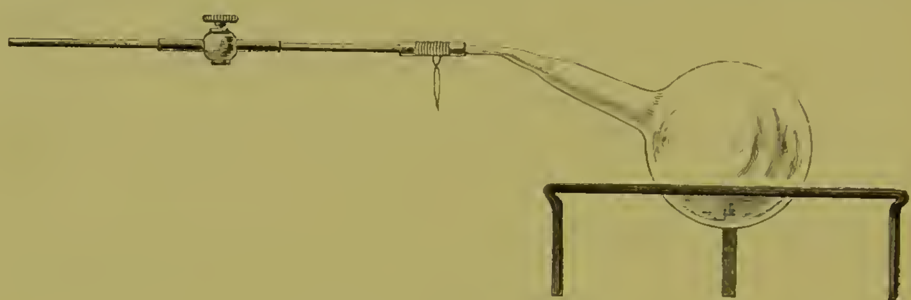

FIG. 13 (FROM PASTEUR).

quantity of water having been previously introduced into the flask. This water is then boiled, and the flask allowed to cool, the air which enters during cooling being previously heated. It is well to boil the water in the flask under pressure, to effect which the free extremity of the platinum tube is connected with a glass tube bent at right angles, which dips into a deep vessel filled with mercury. After boiling for some time under pressure, this tube is detached, and boiling is continued at the ordinary pressure; then the flask is allowed to cool and to become filled with heated air. When the flask is cold the cock is shut and the platinum tube detached. Till it is required it is well to hold the orifice of the brass tube down, in order to prevent dust from falling into it. Before being used this portion is heated carefully in the flame of a spirit lamp.

'A vein or artery of a dog is now opened, the end of the brass tube introduced, and secured in the blood-vessel by a ligature; the cock is then turned on. Blood flows into the flask, and when enough has been obtained the cock is shut and the flask placed in a suitable temperature.'

As a result this blood does not putrefy, and its odour remains quite fresh. There is not even an active absorption of

[1] *Études sur la Bière*, 1876.

oxygen, for after several weeks only 2 or 3 per cent. of that gas was found to have disappeared in a vessel sealed immediately after the blood had been introduced.

Dr. Burdon-Sanderson [1] also found that blood taken from rabbits with suitable precautions, and put into purified flasks covered with cotton wool, remained free from change.

Dr. Roberts,[2] having purified his tubes in the way described, and having thoroughly cleansed his finger, punctured it, and sucked up about two drops into each tube. Of ten tubes prepared in this way, six remained unaltered. This experiment is of little value, partly on account of the imperfect method of experimentation, and partly on account of the small amount of blood obtained.

Mr. Lister [3] took blood from the jugular vein of an ox in the following manner:—

A large glass tube was fixed in the large orifice of one of his double-necked flasks, the interval between the flask and the tube being filled with tightly-packed cotton wool. Over the outer end of this glass tube a cotton cap was applied, and there was a cotton cap as usual over the orifice of the spout. The flask thus arranged was heated in the hot box. The jugular vein of an ox having been exposed antiseptically, was divided, the cotton cap removed from the end of the tube, and the end of the vein slipped over the orifice of the tube. Blood thus flowed through a pure tube into a pure flask. When enough had been obtained the vein was removed and a pure cotton cap immediately applied in its stead. Before coagulation had occurred, various liqueur glasses, arranged as formerly described, were charged from the large flask.

Blood so obtained remained unaltered in the liqueur glasses and in the flask, though kept for six weeks.

Mr. Lister also found that not only blood, but blood and water—a much more putrescible mixture—remained unaltered. (The water was introduced into a large pure flask, and boiled so as to purify it. A portion of blood clot from one of the liqueur glasses was then spooned into the flask, careful precautions being taken against the entrance of living dust.)

In some experiments, performed in a manner to be shortly

[1] *Quarterly Journal of Microscopical Science*, xi. 1871.
[2] *Loc. cit.*
[3] *Microscopical Journal*, 1878.

described, I have found that blood, removed from the healthy living body and placed in calcined flasks or in flasks containing infusion of cucumber, may be preserved for an indefinite length of time without alteration.

Hence blood has no inherent tendency to undergo fermentative changes, nor can oxygen alone induce such alterations.

III.—*Urine.*

Healthy urine was first preserved without alteration by Van der Broeck. The flasks in which it was received were prepared in the manner before described. An animal (dog or sheep) was killed, the abdomen was immediately cut open, and the ureters and urethra having been rapidly tied, the bladder was removed and immersed in the mercury. A heated needle was then introduced, and the bladder was torn, the urine then ascending into the glass. This urine remained pure even after the addition of oxygen or filtered air.

In the same paper in which Pasteur mentions that he has succeeded in preserving blood he states that he has also obtained pure urine. The method is described in his 'Etudes sur la Bière.' The flask with its nozzle and stop-cock are prepared as in the case of the blood; then the free extremity of the brass tube is introduced into the urethra. Urine being passed, the stop-cock is turned, and the urine flows into the flask. Urine thus obtained undergoes no fermentation. 'Elle dépose des cristaux en petite quantité, mais sans se troubler ni se putréfier d'aucune façon.'

In 1871 Mr. Lister succeeded in obtaining and preserving unboiled urine.[1] The method he employs is to wash the meatus urinarius and the glans penis with 1–40 carbolic lotion. A prepared flask is then taken, the cotton cap is removed, the glans immediately applied over the orifice, and urine passed into the flask. A fresh cotton cap is then applied. This urine may, like other fluids, be decanted into liqueur glasses. This experiment was apparently constantly successful, no alteration occurring in the urine in the flasks or in the glasses.

[1] *Transactions of the Royal Society of Edinburgh*, 1875.

I may here state that I have often repeated this experiment with the view of obtaining pure unboiled urine for other experiments, and always with success. I have, however, used the spray, and have thus avoided the necessity of applying the glans penis to the orifice of the flask. The glans having been purified, urine is simply passed in a spray of carbolic acid into a pure flask. This urine passed through the air, but that air, having been acted on by carbolic acid, was inert.

Dr. Roberts has also obtained similar results by passing urine into a pure test-tube, and afterwards charging tubes of the form previously described, by breaking off the capillary end below, and letting the urine flow up. Of eight tubes so obtained, the urine remained unaltered in seven, while in one it putrefied.

Cazeneuve and Livon [1] succeeded in preserving urine in the urinary bladder without the occurrence of any alteration in it.

A ligature was placed around the prepuce of a dog for five hours, in order to have a considerable amount of urine in the bladder. An incision being made into the abdominal cavity at the end of that time, the ureters and the urethra were ligatured, and the bladder was cut out. The bladder was then suspended in the air at a temperature of about 25° C. The wall of the bladder soon dries, and though liquid slowly transudes, that liquid evaporates immediately, and thus the bladder wall cannot putrefy. Urine may be kept thus for several days without undergoing any change, although if the bladder be opened it becomes ammoniacal in twenty-four hours. I shall return to these experiments at a later period.

Thus healthy unboiled urine has no inherent tendency to putrefy, but follows the same law in this respect as boiled urine.

IV.—*Milk.*

In 1859 Hoppe-Seyler attempted to preserve milk pure in the following manner:—[2]

A small funnel was carefully fastened over the teat of a goat. To the lower end of this was fastened a piece of caoutchouc tubing, the

[1] *Revue Mensuelle*, 1877, p. 733.
[2] Virchow's *Archiv.* xvii. (1859).

other end of which was attached to a glass tube below. This glass tube passed down to the bottom of a glass test tube, the upper rim of which was provided with a piece of caoutchouc tubing open above. None of the tubes were heated nor in any way purified. The milk was now withdrawn in a continuous stream, so as to flow for a long time over the edge of the caoutchouc tube till it was quite free from bubbles of air. The test tube was then lowered, and then, while the milk was still flowing, the caoutchouc tube was firmly tied around a thick glass rod.

Milk obtained in this manner, and kept at the ordinary temperature, coagulated in three days. Hoppe-Seyler therefore concluded that milk when shed contains a ferment.

This experiment proves that oxygen is not necessary for the occurrence of fermentation in milk; in other words, it is not the cause of such changes, and therefore, as the tubes were not purified, the cause must either be in the milk itself or be something adhering to the tubes. As I have just stated, Hoppe-Seyler concluded that the cause was inherent in the milk.

Which of these is the true agent is decided by the following experiments performed by Dr. Roberts:—[1]

'A glass tube was drawn out at each end to a narrow orifice. The lesser portion of this was tightly wrapped round with cotton wool and inserted as a plug into a large test tube containing water to the depth of one inch. A cap of cotton wool was also tied over the narrow orifice. The water in the test tube was then briskly boiled, and the boiling was continued almost to dryness. When the apparatus was cold I took it into the cowhouse, and seizing a teat, I pulled off quickly the cotton-wool cap and pushed the narrow point into the duct of the teat. Holding it firmly in this position I milked into the test tube until sufficient milk had been obtained. I then drew away the test tube from the little tube, pressing in the cotton wool around it as I did so, until the latter was entirely withdrawn from the test tube.

'From the test tube I charged ten empty pure tubes' (in the manner described under urine), 'and resealed their capillary orifices: of these ten tubes three remained unchanged, the milk remaining perfectly normal as regards taste, reaction, &c. The other tubes curdled or putrefied in ten days.'

[1] *Loc. cit.*

The method described here is imperfect, but the fact that three tubes remained unaltered absolutely demonstrates that the cause of the fermentation is nothing inherent in the milk itself, but something which it acquires after it leaves the body—that something being particulate, not gaseous.

Mr. Lister[1] describes several series of experiments performed with the same aim. In one of these he succeeded in preserving the milk unaltered.

A number of little tubes were covered with glass caps and shades, and purified in the usual manner. After a rainy day he washed the udder of a cow and the hands of the milkman with water. A wide glass tube connected with an elastic tube was then placed under the nipple (the glass tube had been heated and the elastic tube boiled). This was filled with milk, and then each little tube in succession had a small quantity introduced by relaxing the elastic tube.

Of twenty-four tubes so prepared and charged two remained permanently pure. The results in the other tubes equally demonstrated that the cause of the fermentation of milk is not inherent in the milk, for the milk in each underwent a different change. These experiments will be more fully considered at a later period.

While in the Shetland Islands in the summer of 1880 I performed a series of experiments, which consisted in obtaining the milk under the protection of a spray of carbolic acid. A number of flasks with cotton caps and long necks were heated before leaving London. The udder of the cow and the hands of the milkmaid being washed with carbolic lotion (1–20), the flasks were uncorked and filled with milk under the spray. In doing so the mouth of the flask was held as close as possible to the teat. The cow was restive and would not allow me to do the milking, and therefore the experiment was performed by the milkmaid. When the restiveness of the cow, the inexperience of the milkmaid at antiseptic work, and the dark and draughty cowhouse are taken into account, it will not be surprising that the milk in a considerable number of the flasks fermented; but nevertheless evidence was got, of the same kind as that obtained by Dr. Roberts and Mr. Lister, sufficient

[1] *Microscopical Journal*, 1878.

to disprove the existence of a ferment in the milk when withdrawn from the body.

In order to transfer these flasks to London I had intended to draw out and seal their necks, but I found this impossible, and therefore I soaked pieces of cork in carbolic lotion, inserted them into the mouth of the flask, and covered them with tar —a very inefficient method. During the voyage the milk was much shaken, and some of the corks proved inefficient, as shown by the leakage of the milk.

The following are the experiments, with their results :—

First Experiment.

August 5, 1880.—The udder and teats of the cow and the hands of the milkmaid having been washed with 1–20 carbolic lotion, and a small spray being directed as well as possible over the part, eight purified long-necked flasks were filled with milk, the milk being drawn directly into each flask, which were held as near the teats as possible. Eash flask was re-covered with its cotton cap, and they were then placed in the upright position in a warm room.

August 8.—The milk in these flasks seems unaltered. There is a little cream on the top in each.

August 10.—Ditto.

August 24.—Four of the flasks have undergone change, the change varying in nature in each flask. The other four are perfectly fluid and present the appearance of pure milk.

To-day the corks were inserted.

September 21 (twenty-three days after the transport to London).—Only two flasks now remain pure, the other six having undergone alterations of various kinds.

October 27.—Examined. The milk in two flasks is perfectly normal.

The result of the first experiment was, that after nineteen days four of the milks had undergone alterations of various kinds, while four remained apparently pure. On October 27—*i.e.* after two months and twenty-two days—two milks were still perfectly right, in spite of a sea voyage and great disturbance.

Second Experiment.

(*a*) *August* 10.—Two flasks were filled after washing the udder of the cow and the milkmaid's hands with carbolic acid (1–20). No spray used.

August 24.—The milk in one of these flasks has undergone alterations; the milk in the other is perfectly pure.

September 21.—Both milks have coagulated and are undergoing changes.

(*b*) *August* 10.—After the spray employed in experiment (*c*) had been stopped, two flasks were filled without it.

August 24.—I am doubtful whether these are pure or not; I think they are not.

September 21.—Changes have occurred in both.

(*c*) *August* 10.—Seven flasks were filled under the spray as usual.

August 24.—Three of these milks have altered; four are still apparently pure.

September 21.—Three still remain apparently pure: four have undergone change.

October 27.—Examined. Three still pure.

On December 24 I opened one of these flasks, and found a slightly suety smell but a perfectly sweet taste, and the milk presented the appearance of normal milk. I examined it microscopically—no organisms. I have stained some specimens of this milk (Plate V. Fig. 33), and it will be seen that no organisms whatever are present, and this in unboiled milk kept for more than five months.

The result here is that three out of seven of the flasks filled under the spray have remained permanently pure, while all of those filled without the spray have ultimately undergone fermentative changes.

Third Experiment.

(*a*) *August* 16.—One flask filled without spray and without previous application of carbolic acid to the hands or teats.

August 24.—This milk has coagulated.

(*b*) *August* 16.—One flask filled without the spray and without washing the teats with carbolic acid. The hands of the milkmaid, were, however, purified.

August 24.—This milk has undergone fermentation, having separated into two layers—the upper clear, the lower thick but not coagulated.

(c) *August* 16. Two flasks were filled without the spray, but after washing both the teats and the hands with carbolic lotion.

August 24.—One of these is doubtful, the other apparently unaltered.

September 21.—Fermentative changes are occurring in both.

(*d*) *August* 16.—Six flasks filled under the spray as usual.

August 24.—All these are apparently unchanged.

September 21.—Four have undergone some fermentative changes. One is doubtful. One is still pure. Three of the corks have not fitted perfectly.

October 27.—Examined. One still pure.

The whole result is, that of twenty-one flasks filled under the spray, six remained permanently unaltered, and that after having been exposed in a manner which sufficiently explained the occurrence of fermentation in some of the others.

Up till August 24th no less than fourteen of these milks had remained apparently unchanged, while similar specimens taken without any precautions had undergone alteration.

I have still in my possession (June 1881) four of these six flasks, and the milk in these still remains perfectly pure and free from fermentative changes.

From all the facts narrated I think it is absolutely certain that milk has no inherent tendency to undergo fermentation of any kind, and that the cause of the fermentation is not the gases of the air, but solid particles which the milk meets with after it is drawn from the cow.

V.—*Egg Albumen.*

The difficulty experienced by Schroeder in preserving boiled white and yolk of eggs will be remembered.

Van der Broeck introduced an egg into the mercury arranged as formerly described, broke the shell with a heated iron rod, stirred up the contents with a similar rod, and then allowed them to ascend into the glass. This egg albumen remained pure, even after subsequent addition of oxygen or of filtered air.

Gayon[1] found that some eggs may be preserved unaltered, while others undergo change. He supposes that in the latter

[1] *Comptes Rendus*, lxxvi. lxxvii.

case the causes of putrefaction entered as the egg passed through the oviduct. Such an idea is, however, hardly tenable.

Roberts has shown by experiments similar to those previously described that egg albumen has no inherent tendency to undergo fermentation.

He proceeded in the following manner:—Eight sterilised tubes were prepared containing pure water. 'A fresh egg was fixed in a convenient support, and a small piece of the shell was chipped off, care being taken to leave the subjacent membrane uninjured; then a sterilised bulb was taken, and the capillary portion immersed for a few seconds in boiling water, in order to destroy any adherent septic particles. The sealed end was then rapidly snipped off and the capillary portion plunged into the interior of the egg. About 2 gm. of the albumen were then sucked up by the mouth into the bulb. When this was accomplished the bulb was quickly withdrawn and its capillary end sealed in the flame.'

Six of these eight tubes remained unaltered for seven months.

Of a second series of seven tubes similarly charged and kept for two months, five remained unaltered. That is, of fifteen tubes filled, eleven remained pure.

I may refer to an experiment which I did for another purpose, accepting as true the view that egg albumen had no inherent tendency to undergo fermentation, and which proves the truth of that view.

On July 7th, 1880, I took four purified beakers and four fresh eggs. These eggs were washed with carbolic lotion (1–20), and were then broken, one into each beaker, under the spray.

One of these beakers was covered with its cotton cap, and placed in an incubator kept at the temperature of 98° F.

On July 20th no change whatever had occurred. The other flasks were used at once for various experiments. Into one a special form of organism was introduced, and here only this one form of organism developed, with the production only of a special kind of fermentation.

Hence egg albumen has no inherent tendency to undergo fermentative changes.

VI.—*Vegetable Tissues.*

Dr. Roberts has also experimented on the solid tissues of the turnip, potato, orange, and tomato, with similar success.

The following is his method for turnip :—

'A sterilised tube containing water was nicked with a file near the base of the capillary part, where the tube had a diameter of about two millimetres. A fresh oblong turnip was then fractured across, and the tube, snipped off at the nicked point, was quickly thrust into the substance of the turnip. A narrow cylinder of turnip about an inch long was thus forced into the column of water in the tube. The tube was then detached, and its end sealed with melted sealing-wax.' Of

14	tubes	thus charged with	turnip	10	were successful;
7	"	"	potatoes	4	"
8	"	"	orange	8	"
3	"	"	tomato	3	"

Ferments which induce changes after death are therefore not present in living vegetable tissues.

VII.—*Animal Tissues.*

Some years ago experiments were made by Billroth[1] and Tiegel[2] with the view of ascertaining whether the living tissues did or did not contain the causes of putrefaction. Having killed an animal, they opened its body rapidly, and removed with heated implements various portions of tissue such as liver, spleen, kidney, &c., and immediately dropped this into heated paraffin. They supposed that by this means any dust which fell on the tissue in its transit from the body to the flask would be destroyed by the hot paraffin, while this heat would not penetrate into and act on the interior of the tissue. At the same time the organs would be protected from air or dust by the paraffin.

They found that many portions of the body preserved in this way, notably the liver and spleen, underwent putrefaction rapidly, and they therefore concluded that the causes of this putrefaction were present in the living blood and tissues.

[1] *Coccobacteria septica.* [2] Virchow's *Archiv.* lx.

These experiments were repeated by Dr. Burdon-Sanderson, who obtained similar results and adopted the same views.

If, however, we look at the method, we shall find several objections to it. Thus, heated paraffin must be looked on as dry heat; it does not moisten solid particles in contact with it. Now it has been shown that dust, if kept dry, may be heated even to 300° F. without losing its power of causing fermentation. Further, paraffin solidifies at about 136° F., or even lower, and therefore paraffin, merely at its melting point, is not likely to be hot enough to destroy all septic particles. Further, during the cooling of the paraffin heavy particles of dust may fall into it and sink on to the tissue. Then, also, on the sides and bottom of the vessel is coarser dust, which likewise may not be destroyed.

But, again, paraffin is very apt to crack, and after cooling small cracks may occur which admit moisture and dust. To obviate this risk the paraffin has been covered with oil; but even here the oil becomes laden with dust and passes down through the cracks.

And, lastly, the knife, before dividing the tissue, compresses the vessels and forces the blood out of them, and thus, when these vessels are cut, air is sucked in, and this air carries its dust with it quite out of reach of the heat of the paraffin.

In December 1877 I commenced a series of experiments on this subject, and these have been continued at intervals since that time.

The first experiment was an imitation of those of Billroth and Tiegel (only it was performed antiseptically), and yielded conflicting results. Thus the liver and kidney putrefied, while the spleen, muscle, and mesentery remained unaltered.

This being the case, I determined to abandon this method entirely, and to see if some definite conclusion might not be arrived at in some other way. The following is a description of the method I have employed:—

A number of beakers, each provided with a cotton cap, were purified by heat, somewhat after Mr. Lister's method, and into each vessel about one-fourth of its volume of pure turnip infusion was introduced from one of the double-necked flasks (Fig 6, p. 19). This was done under the spray, and the cotton caps were then reapplied. These beakers

were placed in an incubator, and kept at a temperature of 98° F. for three or four days. At the end of that time the turnip infusion was clear and unaltered, and the flasks were therefore considered ready for use.

On January 6th, 1878, four beakers having been thus prepared, and six beakers containing melted paraffin being also at hand, a healthy rabbit was used for the following experiment.

The skin and hair of its abdomen having been thoroughly washed with 1–20 carbolic lotion, the animal was killed by a blow on the back of its neck, and the abdominal cavity was rapidly opened, under a fine spray of carbolic acid, with purified and heated instruments. Portions of its organs and tissues were rapidly cut out and introduced into the beakers, which were opened in the spray.

Fig. 14.

Into the four vessels containing the pure turnip infusion portions of liver, spleen, kidney, and muscle respectively were introduced, and the caps having been reapplied while the flasks were still in the spray, they were then placed in an incubator (see Fig. 14).

Into the six flasks containing melted paraffin portions of liver, kidney, spleen, muscle, mesentery, and vena cava, with its blood, were dropped also under the spray. The paraffin was left to solidify, and the vessels were then placed in the incubator.

All those portions of organs introduced into the turnip infusion remained permanently pure and free from putrefaction.[1]

Of the paraffin beakers, two (muscle and vena cava) remained without change; while the other four (liver, spleen, kidney, and mesentery) putrefied.

In this experiment we have in the first case a series of

[1] On December 24, 1880, I killed a rabbit and preserved its organs in the way described here. Fig. 34, Plate V., is drawn from a specimen taken from the beaker containing the spleen, and stained. It will be seen that no organisms whatever are present.

beakers heated so as to destroy the activity of the dust adhering to them, and that this was effectually done was proved by the fact that the turnip infusion introduced into them underwent no change, although, as has been amply shown in the foregoing experiments, had ordinary unheated dust been present, this infusion would have undergone fermentation.

Further, the portions of the tissue are transferred from the body to the beaker without the possibility of acquiring living dust, for, as we have seen before, a spray of carbolic acid in an ordinary atmosphere is able to destroy the fermenting power of the dust. Such being the case, if the tissue, taken with all precautions undergo putrefaction, it is possible that the causes of this fermentation were present in it while in the living body—the degree of probability depending of course in great measure on the known skill of the experimenter. But if no change occurs, it is proof positive that there were no causes of change present in the body. In other words, as these unboiled tissues remained unaltered, it is quite certain that they have no *inherent* tendency to undergo fermentation even when freely exposed to air.

I used the turnip infusion partly because I wished to know whether the beakers had been thoroughly purified, and partly in order to keep the tissues moist, for I had found in a former experiment that they dried too rapidly in the open-mouthed vessel if no fluid were present. Since that time I have used cucumber fluid, as being more putrescible.

Further, by the use of these infusions the conditions favouring fermentation are greater, for we have here a boiled highly putrescible infusion of turnip, and an unboiled, if possible still more putrescible, infusion of meat, as well as the meat itself. It were hardly possible to provide more favourable conditions for fermentation. Nevertheless no change occurred.

I may here point out the light thrown by these experiments on the cause of the want of success in the paraffin experiments. In the first attempt which I made with the paraffin any of the supposed causes of failure might have been in operation, but in the experiment just narrated the entrance of air laden with septic dust into the blood-vessels is excluded because the operation was done in a spray of carbolic acid. Therefore the failure in

the four vessels must have been due to dust in the paraffin, or to cracking of this after solidification.

But, it may be said, the absence of putrefaction in the beakers was due to the action of the carbolic acid on the tissue. This, however, is not the case, for the following reasons:—

In a preliminary experiment I touched the outside of the flask (which was of course covered with impure dust) with one of the portions of the tissue, and afterwards introduced this piece into the flask, and in it putrefaction occurred rapidly. Again, the fact that four paraffin flasks went wrong (the organs being there also subjected to the action of the spray) shows that this had no influence. Again, when the gall-bladder is wounded fermentation often occurs. This latter fact is illustrated by the following experiment:—

A medium-sized rabbit was killed by a blow on the nape of the neck. The abdomen had been washed beforehand with 1–20 carbolic acid lotion, and was now rapidly opened under the spray. Into seven beakers containing pure cucumber infusion, two pieces of liver, one piece of kidney, one piece of spleen, one of muscle, and one of mesentery were introduced. In cutting out the liver the gall-bladder was injured.

Four weeks later, five beakers were unaltered, the two which had fermented being those containing the pieces of liver, which indeed had undergone fermentation within twenty-four hours.

I have since met with several similar instances.

Further, if putrid matter be injected into the jugular vein of the animal a few minutes before death, all the tissues removed and preserved in the manner described undergo putrefaction.

I have repeated these experiments many times with like results,[1] and I therefore conclude that the *tissues* of the healthy

[1] On two occasions I have found that the apparently healthy living tissues, preserved by the method before described, underwent fermentation and organisms developed in them. In one case the kidney alone of all the organs taken, and in another both kidney and liver, underwent fermentation with development of organisms, and as I was very careful in performing the experiments, I do not think that this could have occurred from any error in experimentation, and therefore I conclude that the causes of fermentation (micro-organisms, as we shall afterwards see) were present in the healthy circulating

living body, like the fluids, contain no ferment capable of causing putrefaction after death, and remain pure in flasks so long as the dust of the atmosphere is excluded. (In some instances the heart with its contained blood was also removed, and remained, like the other tissues, unaltered. Rabbits and cats were the animals used for the experiments.)

Somewhat similar experiments were published in 1878 by Chiene and Ewart, and they yielded similar results.[1]

Quite recently,[2] Rosenbach mentioned experiments on this subject performed by Meissner. Meissner was able to preserve the internal organs of cats and rabbits in contact with boiled water and pure air, for two to three years, without the occurrence of any putrefactive change. He was also successful in preserving the blood of mammalia, human urine, and goat's milk. The experiments were done with strict aseptic precautions, and led him to conclusions similar to the above.

Such, then, are the chief facts at present known with regard to boiled and unboiled fluids and tissues. We shall add much to them, and to the support which they give to the views here expressed, when we come to consider more minutely what is the nature of the particles which cause putrefaction.

On reviewing the mass of evidence before us we have it distinctly shown that boiled fluids and tissues have no inherent tendency to undergo fermentative changes; that oxygen, whether pure, nascent, or mixed with nitrogen in the proportions present in air, cannot cause fermentation, if only the air be previously passed through such a liquid as sulphuric acid, be heated strongly, be filtered through cotton wool, be made to enter very slowly into the flask containing the fluid or allowed to deposit its dust by gravitation, or be previously acted on by carbolic acid.

blood. That an organism may be present in an active state in the circulating blood need not be a matter of surprise, and need not therefore lead us to the conclusion that they are always or even generally there, especially as one single organism would be sufficient to account for the result in each of these instances. It is indeed surprising that organisms which must now and then enter the blood are so rapidly and surely destroyed.

[1] *Journal of Anatomy and Physiology.*

[2] *Deutsche Zeitschrift für Chirurgie*, xiii. 344.

Thus the material in the air which causes putrefaction is not a gas, for that would be continuous, and would not be removable by filtration or by rest; but it is something discontinuous, something heavier than air, something particulate. These particles may be deprived of their power of causing fermentation by the action of chemical substances, such as sulphuric and carbolic acids, and also by being subjected to a high temperature. As they are completely destroyed by heat (as shown by Tyndall), they are probably of an organic nature.

And it is not that by boiling these fluids an inherent tendency to ferment has been destroyed, for, as we have seen, they possess no such inherent tendency. For not only do unboiled fluids and tissues outside the body fail to putrefy when protected carefully from dust—they also undergo no change, as indeed necessarily follows from the foregoing, when confined in natural or artificial cavities in the living body. Who is not acquainted with the behaviour of blood when extravasated into the tissues or cavities of the living body so long as it is not exposed to the outer world? We all know what a large amount of effused blood may be present about the ends of a fractured bone without decomposition occurring in it, and the same is the case in the hemorrhages into joints in hemophilia, hemorrhages within the skull, &c. And we also know what frequently happens if we cut into any of these extravasations and admit dust-laden air into them. The blood which we found odourless, and it may be clotted, may become in a few hours a foul-smelling liquid; it has, in fact, putrefied, just as it may do when kept in a flask without exclusion of dust.

And just as in the case of blood, so with other fluids. Hydrocele and serous effusions remain unaltered so long as they are kept from the dust. Examine the pus from a chronic abscess, and even though that abscess be connected with carious bone, it will be found to be odourless and bland, and if carefully received into pure flasks, will, just as in the case of blood, remain odourless and apparently unchanged for an indefinite length of time. (I shall give later on the explanation of the cases where the pus of acute abscesses, when let out, is found to have a foul smell, as is sometimes the case in acute necrosis.)

And not only is this the case with fluids, it is also the case with tissues in the living body. In a fracture many portions of the tissue are cut off from their vascular supply, or killed by the violence causing the injury, and yet they do not decompose; they are not separated as sloughs—they disappear by absorption. Yet if the same injury be not subcutaneous and the injured parts be exposed to ordinary air, they putrefy, and come away in a few days as sloughs.

So in infarcts in internal organs, the tissue in the region of the infarct dies, but does not putrefy—does not slough; while when death of the integuments occurs, putrefaction and sloughing follow, for here the dead tissue is exposed to the dust of the atmosphere.

Similarly, in the case of wounds, when a piece of skin is cut away and an open sore is left, the blood and serum which collect in that sore ferment, in all probability putrefy, because the air admitted to them was not heated air, not filtered air, was air which had not been acted on by suitable chemical substances.

The causes of fermentation are therefore solid particles, probably of an organic nature, which are present in varying quantities in the surrounding air, and which are deposited as dust on all surrounding objects.

It is thus evident that in order to prevent putrefaction it is only necessary to prevent the access of these particles, or, if this cannot be done, to destroy their fermenting power in some way or other before they reach the wounds—as, for instance, by the use of carbolic acid.

It is on this principle that *Aseptic Surgery*,[1] as introduced by Mr. Lister, is based.

[1] The term 'aseptic' is the best to indicate this form of antiseptic surgery, because, as we shall see, there are many different forms of treatment which come under the term 'antiseptic,' while this is the only one which can truly bear the name 'aseptic.' In other words, there are many methods by which the occurrence of putrefaction is more or less interfered with, but they all act on a more or less imperfect principle, with the exception of that introduced by Mr. Lister, which, founded on a true principle, attains the ideal of results—viz. a complete absence of putrefaction—an asepsis. His method, then, is best designated by the term expressing its result—Aseptic.

CHAPTER III.

ASEPTIC SURGERY—MATERIALS EMPLOYED.

Problems to be solved in order to keep a wound aseptic: Carbolic acid—*Carbolic lotions—Pure carbolic acid—Solution in methylated spirit—carbolic oil—Carbolic acid and glycerine*: Spray producers: Catgut—*Carbolised catgut—Mr. Lister's carbolised chromic catgut—Dr. MacEwen's chromic catgut—Catgut trough and pocket case*: Carbolised silk: Protective: Carbolic gauze—*Composition—Method of preparation—Von Bruns' gauze*: Macintosh: Sponges: Boracic acid—*Boracic lotion—Boracic lint—Boracic ointment*: Salicylic acid—*Salicylic acid cream—Salicylic ointment*: Chloride of zinc: Iodoform: Carbolised cotton wool.

Aseptic surgery is based on the principle first enunciated by Mr. Lister, and indicated in the preceding pages; viz. the *exclusion* of active ferments from the discharges of wounds.

Theoretically, this is the ideal form of antiseptic surgery, for here, supposing that the attempt is successful, the causes of putrefaction do not enter the wound in a state capable of producing fermentation, and therefore decomposition of the discharges, or of dead portions of tissue, &c., cannot possibly occur.

The problem which Mr. Lister sought to solve may be shortly stated as follows:—

On all objects in the external world septic dust is present—on the skin of the patient, on the hands of the surgeon and his assistants, on all instruments, in water, in the air, &c.; and when a wound is made any introduction of this dust must be carefully avoided. Then after the wound has been made, care must be taken to prevent its entrance during the after-treatment. Some sort of dressing must be provided which shall prevent its passage in an active state, and at each change of this dressing the problem is the same as at the time of infliction of the wound. Such being the question

at issue, I must now proceed to the modes in which it has been answered.

I shall first enumerate the substances employed in aseptic surgery.

Carbolic Acid is the antiseptic employed to destroy the particles in the air and on surrounding objects which give rise to putrefaction. It is obtained in the solid state and of extreme purity from Bowdler and Bickerdike, Church, Lancashire, who give it the name of Absolute Phenol. It is used in various forms.

The *Carbolic Lotions* used are of two strengths—1 in 20 and 1 in 40; one part of crystallised carbolic acid dissolved in 20 or 40 parts of water respectively. The solution is kept in a stoppered bottle in order to avoid evaporation of the acid. It ought to be quite clear; when it is not so, and more especially when globules of oily matter are present, it is impure, the oily particles consisting of cresylic acid. It is a mistake to add alcohol or glycerine to aid the solubility of the acid, because these substances hold the acid more tenaciously than water, and it is thus not so potent for producing an instantaneous effect.

Undiluted Liquid Carbolic Acid may in some cases be required. This is obtained by liquefying the crystals by the addition of a few drops of water. This is chiefly used for injecting nævi, varicose veins, &c.

A solution of carbolic acid in *methylated spirit* or in rectified spirit, in the proportion of 1–5, is used for the purpose of purifying wounds inflicted some twenty-four or thirty-six hours before coming under treatment.

Carbolic Oil is employed in various proportions, generally 1 in 5, 1 in 10, and 1 in 20, consisting of carbolic acid mixed with olive oil in the foregoing proportions.

Carbolic oil 1-5 is but rarely used, though it is occasionally applied as a dressing to foul wounds, for the purpose of purifying them. It is chiefly known as the solution in which catgut is permanently preserved.

Carbolic oil 1–10 is used as a dressing for wounds in the neighbourhood of the anus, penis, &c.

Carbolic oil 1–20 is used for oiling catheters or other in-

struments before introducing them into the bladder. Carbolic acid of this strength does not seem to be too irritating for the

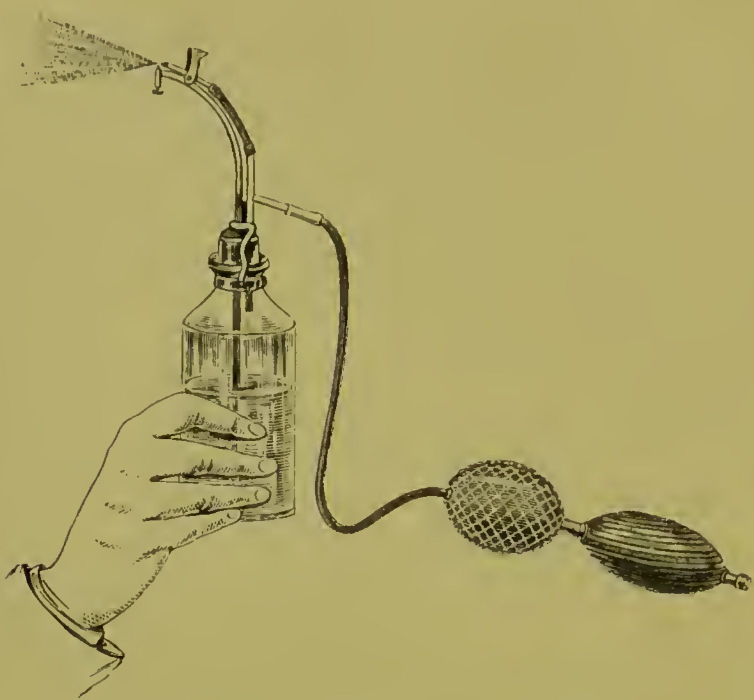

FIG. 15.—HAND SPRAY PRODUCER.

mucous membrane of the urethra, while it apparently secures against the introduction into the bladder of matters which are capable of causing putrefaction.

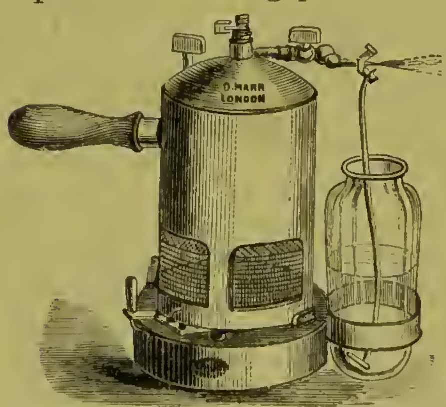

FIG. 16.—THE ORDINARY STEAM SPRAY PRODUCER.

There are various objections to these oily dressings. The chief are, that the carbolic acid is very rapidly washed out by the discharge, and that they are very dirty, and soon spoil india-rubber tissues.

The latter disadvantage is got rid of by the use of *carbolic acid and glycerine* in the proportions of 1–5 and 1–10. This is now employed as a dressing in the circumstances in which the oil has been generally used.

A *spray* of carbolic acid is generally employed in order to purify the atmosphere. This is obtained by driving a rapid

current of air or steam through a horizontal tube so as to pass over the orifice of a more or less vertical one. In this way a vacuum is produced in the vertical tube, and the fluid at its lower end rises, and is expelled from the orifice in the form of spray. We have two forms of spray: one in which air is

FIG. 17.—STEAM SPRAY PRODUCER, SHOWING THE LAMP AT PRESENT IN USE.

It consists of a small flame, which plays on a plate of metal attached to a hollow central tube containing a wick, and perforated by holes at the top. The heat is communicated to the wick —the spirit volatilises, and burns as it escapes from the top of the tube. The hole in the plate allows the flame of the small wick to pass up and light the spirit vapour, which passes out through the holes at the top of the central tube. The cap, which is placed over the lamp when not in use, and the boiler and vessel for the carbolic acid, are indicated by dotted lines.

driven over the vertical tube—hand or foot sprays; and the other in which steam is employed—steam sprays. The hand or foot sprays produce a somewhat coarse spray, and the force required is such as soon to exhaust the individual employed. They are therefore very uncertain implements, and have now entirely given place to the steam sprays, where there is a steady current as long as the water in the boiler lasts. When the hand sprays are employed 1–40 solution is placed in the bottle.

In the steam spray the 1–20 solution is used because the steam, mixing with the solution, reduces its strength to 1–30 or 1–35.

I need not describe here the steam spray apparatus, more especially as it can be easily obtained, of Mr. Lister's pattern, from David Marr, 27 Little Queen Street, Holborn. These sprays consist of the following parts:—A boiler which contains water, and which is heated by a spirit lamp placed beneath it. The steam issues through a tube placed at an angle to another more upright one, through which the carbolic acid lotion 1–20, which is placed in the glass retort, is sucked up. This 1–20 lotion, mixing with the steam, makes a solution of 1–30 to 1–35. The carbolic acid solution passes through a sponge at the bottom of the upright tube, which filters it, and thus prevents the minute orifice of the tube from becoming choked up by coarse particles. These sprays are of various sizes, and the largest ones have two nozzles, which may be used singly, or together where the field of operation is large as in ovariotomy.

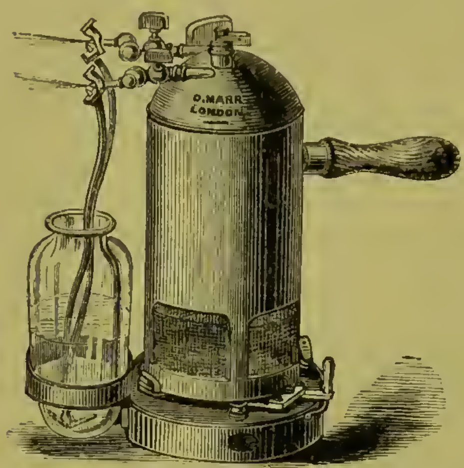

Fig. 18.—Large steam spray producer with double nozzle for ovariotomy, etc.

The *catgut* used for tying the vessels is prepared in the following manner:—Ordinary catgut as obtained from the shops, when introduced into blood-serum, soon swells up and becomes weak. At the same time it is very slippery, and a knot will not hold; and when placed in the tissues it very rapidly becomes absorbed. With the view of obviating these defects it is prepared by placing it in a solution of carbolic acid in oil (1–5) with a very small quantity of water (10 per cent.) diffused through the oil. As the water ultimately falls to the bottom, a few pebbles are placed on the bottom of the vessel, on which the catgut rests. It does not then come in contact with the water. As a result of keeping it in this solution, the catgut first becomes supple and soft, but afterwards harder and firmer, and is then able to resist the action of the fluids and

tissues for a considerable time. It is kept in this fluid for six or eight months, and is then transferred to the ordinary 1–5 oily solution. The longer it is kept in this solution the better it becomes. The rationale of this method will be found in Mr. Lister's paper in the 'Lancet' of Feb. 5, 1881.

Quite recently [1] Mr. Lister has published a new method of preparing catgut, by means of which a stronger article is obtained, and one not absorbed nearly so soon as the old kind. It also possesses an advantage over the old kind, in that it disappears by erosion of the surface, and does not become infiltrated with cells, as is the case with the ordinary forms. It thus remains as a firm constricting band, till it is completely replaced by new tissue. I cannot do better than quote Mr. Lister's description of the method by which this gut is prepared: 'I dissolve one part of chromic acid in 4,000 parts of distilled water, and add to the solution 200 parts of pure carbolic acid, or absolute phenol. In other words, I use a 1-20 watery solution of carbolic acid—only that the carbolic acid is dissolved, not in pure water, but in an exceedingly dilute solution of chromic acid. But, minute as is the quantity of the chromic acid, it exerts, when in conjunction with carbolic acid, a most powerful effect upon the gut. The first effect of the addition of the carbolic acid to the chromic solution is to change its pale yellow to a rich golden tint. But if the liquid is allowed to stand without the introduction of the catgut, it changes in the course of a few hours to a dingy reddish-brown, in consequence of some mutual reaction of the two acids; and a considerable amount of reddish grey precipitate is formed. If, however, catgut about equal in weight to the carbolic acid is added as soon as the ingredients are mixed, the liquid retains its brightness, and the only change observed is the gradual diminution of the depth of the yellow colour; the precipitate, which I presume still occurs, taking place into the substance of the catgut. As soon, therefore, as the preparing liquid has been made, catgut equal in weight to the phenol is introduced into it. If you have too large a proportion of catgut, it will not be sufficiently prepared; if you have too small a quantity, it may run the risk of being over-prepared. At the end of forty-eight

[1] *Lancet*, Feb. 5, 1881.

hours the chromic element of the liquid has nearly spent itself, and precipitation is complete. The catgut is then taken out of the solution and dried, and when dry placed in 1–5 carbolic oil: it is then fit for use. . . . The preparing liquid causes a certain amount of softening of the catgut, and if it is introduced in loose hanks, this will tend to produce a little uncoiling of the twisted cord, and a still greater degree of uncoiling will take place during drying. It is of very great importance that this should not occur, because it involves weakening of the thread, and that in different degrees in different parts; and this may lead to the gut giving way when you subject it to a strain. The catgut then should be prepared on the stretch, both when it is put to soak and when it is put to dry.

'I need not enter into the mode in which this can be done by the manufacturer. I may only say this, that the surgeon who wishes to prepare it himself may do it in different ways. For instance, he may take two large test-tubes, one a little larger than the other, and he may wind the catgut on the smaller tube, fixing one end by sealing-wax, winding it round, and then bringing it up again, and fixing the other end with sealing-wax at a higher level than the liquid will reach, putting sufficient liquid into the larger test-tube, and introducing the smaller test-tube with the catgut wound round it, with a little shot to keep it down in the liquid. After forty-eight hours, he takes out the smaller test-tube, and leaves it till the catgut is completely dry. I merely mention this as an illustration, and also as furnishing a hint to some surgeons in private practice who may desire to prepare the catgut themselves; or a couple of gallipots, one larger than the other, will do just as well. But, as I have said, the principal uncoiling takes place during drying; and for all ordinary purposes a sufficiently good article is got by putting the catgut loose into the liquid, and making it dry on the stretch by tying the ends of each hank to two fixed points in a room.' Erosion of this catgut does not begin till about a fortnight after its introduction into the tissues.

Dr. MacEwen has lately brought forward a somewhat different method of preparing catgut.[1] 'These ligatures are prepared by making, first, a watery solution of chromic acid, one

[1] *British Medical Journal*, Jan. 29, 1881.

to five; then one part of this solution is added to twenty of glycerine. This forms a dark greenish compound, in which the hanks of catgut are inserted and retained for seven or eight months, the bottle containing them being occasionally shaken. At the end of this time the catgut acquires a semi-translucency, and has a dark colour like preserved ginger. It is then ready

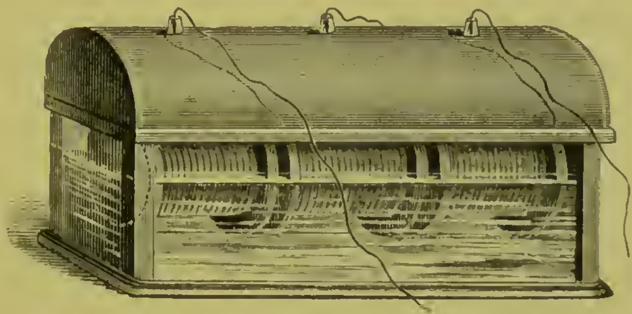

FIG. 19.—TROUGH FOR CATGUT.

for use, and is stored in a solution of carbolic acid and glycerine (one to ten).' Ligatures so prepared begin to soften on an average about the fourteenth day, and are more or less completely absorbed the twentieth day.

A very convenient method of keeping catgut for use in hospital is to wind it round reels, say three, each holding a different thickness of gut, which are suspended in a vessel

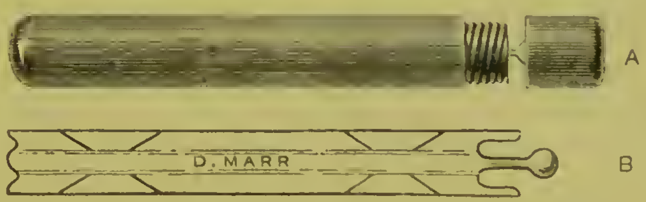

FIG. 20.—LISTER'S POCKET CATGUT HOLDER.

A, Holder complete. B, The reel on which the catgut is wound.

containing carbolic oil 1–5 (see Fig. 19). The lid of the box is perforated with holes through which the ends of the catgut pass. In this way the gut may be taken directly from the oily solution without any trouble.

In order to have a supply of gut always at hand, Mr. Lister has devised the catgut holder shown in the accompanying figure (Fig. 20). The gut is wound on a reel which is carried in a German silver case. There is no necessity to fill this case with

carbolic oil, as is done by some surgeons. Sometimes the case is combined with a caustic holder at the other end.

Carbolised Silk is often used for sutures, and is prepared as follows:—Nine parts of beeswax and one part of carbolic acid are melted together. Silk thread of various sizes is steeped for some minutes in this mixture, till it is thoroughly impregnated with it. As the thread is taken out, it is drawn through a cloth in order to remove the superfluous wax. The wax holds the carbolic acid, makes the thread more useful, and fills up to some extent its interstices, thus preventing it from becoming soaked with fluids. The carbolised silk thus prepared is kept permanently in stoppered bottles, or wrapped in carbolic gauze. It must not be steeped for any length of time in the lotion before being used, because the threads become opened out. If the thread be properly kept, the interior is aseptic or even antiseptic, and passing the thread through the fingers moistened with carbolic lotion, or a momentary immersion in 1–20 carbolic solution, is sufficient to destroy any septic dust adhering to its exterior.

In order to protect healing wounds from the irritation of carbolic acid a special material is employed termed the *Protective.* This consists of oiled silk coated with copal varnish. When this is dry a mixture of one part of dextrine, two parts of powdered starch, and sixteen parts of cold watery solution of carbolic acid (1–20) is brushed over the surface. The rationale of this method of preparation is the following:—Oiled silk alone is better for the purpose of a protective than gutta-percha tissue, because carbolic acid does not so readily pass through it. It does, however, do so, and therefore copal varnish, which is almost absolutely impermeable to carbolic acid, is added. As, however, the fluid collects on this as on a duck's back, leaving intervals between each drop on which dust may fall and escape the action of the acid, the dextrine solution is added, and the result is, that when moistened the whole surface of the protective remains uniformly wet. The use of the carbolic acid in the dextrine solution is not to add any carbolic acid to the protective, but because it is better than water for enabling the dextrine to adhere to the varnished oiled silk. For the same reason the powdered starch is added. The original carbolic

acid flies off very quickly from the protective, leaving a material containing no antiseptic in its substance.

Carbolic Gauze is the material generally employed as a dressing to guard against the entrance of causes of fermentation into a wound after an operation. The gauze used is ordinary fine unbleached tarlatan washed and dried. There are various methods of impregnating this gauze with carbolic acid. I will give Mr. Lister's description of a simple method, which can be used in any hospital, and which was recently employed during the Russo-Turkish war for making fresh gauze in camps.[1] The mixture employed for charging the gauze was originally one part of carbolic acid, five parts of resin, and seven parts of paraffin. Lately the formula has been changed to one part of crystallised carbolic acid, four parts of common resin, and four parts of paraffin. These materials, mixed together, are added to an equal weight of unprepared gauze. 'In order to charge the gauze, the paraffin and resin are first melted together in a water bath, after which the acid is added, and blended by stirring. The object now is to diffuse this melted mixture equally through the cotton cloth, and for this purpose two things are requisite, viz. that the cotton be at a higher temperature than the melting point of the mixture, and that it be subjected to moderate pressure after receiving it. The cotton cloth, a yard wide, is cut into six-yard lengths, and these having been folded so as to be half a yard square, are placed in a dry hot chamber, formed of two tin boxes placed one within the other, with an interval to receive water, which is kept boiling by fire or gas beneath, the upper edges of the boxes being connected and provided with an exit pipe for the steam. There is also a glass tube arranged as a gauge of the amount of the water, and the chamber has a properly fitting lid The bottom of the chamber is strengthened with an iron plate, to enable it to bear the weight used for compressing the gauze when charged. There is a piece of wood about two inches thick nearly fitting the chamber, covered with sheet lead, so as to make it about as heavy as a man can lift by means of two handles in the upper surface. The weight is heated along with the cotton, and is put first into the chamber so as to leave

[1] See *Lancet*, March 13, 1875.

the cotton loose for the penetration of the heat, which occupies two or three hours. The cotton when heated is taken out of the chamber along with the weight, and placed in a wooden box to protect it from the cold. (It would be better to have a second hot chamber for this purpose, since in cold weather the cotton is apt to be too much cooled in spite of the protection of the wooden box.) The heated gauze is then at once charged with the melted mixture of carbolic acid, resin, and paraffin, in quantity equal to the weight of the cotton fabric (or slightly less), and in order to diffuse the liquid as equally as possible, it is sprinkled over the gauze by means of a syringe, with a number of minute perforations in its extremity, the body of the syringe and the piston-rod having each a wooden handle to protect the hands of the workman from the heat. The syringe is constructed to hold half the quantity of the mixture required for charging one piece of cloth. One folded piece being placed at the bottom of the hot chamber, its upper half is raised and turned aside, and one syringeful is sprinkled over the lower half. The upper half is then put back into position, and another syringeful thrown on. The same process is repeated with all the other pieces of gauze, after which the weight is put into the chamber to compress the charged cotton, and the lid applied. An hour or two are then allowed to elapse, to permit the complete diffusion of the liquid, when the material is fit for use. The apparatus above described can be made by a common tinman for about 10*l.*' Fig. 21 shows the apparatus employed in the Glasgow Infirmary.[1]

As the muslin is the dearest item in the gauze Mr. Lister has suggested that the dressings should be washed and the gauze recharged. The larger dressings are therefore kept and sent back to the manufacturer, who washes and recharges them. This recharged gauze can then be used as loose gauze in future dressings.

In this gauze the carbolic acid is the only active agent; the resin is used to hold the acid—*i.e.* to prevent it from being washed out too soon by the discharge—while the paraffin is employed to lessen the adhesiveness of the resin. The gauze

[1] See 'Practical Papers on the Materials of the Antiseptic Method of Treatment.' By George Beatson. *Glasgow Medical Journal*, March 1880.

ought to be kept in a tin box, closing tightly to prevent evaporation of the carbolic acid. It is used either in the form of loose gauze or folded dressings and bandages.

A great many different ways of preparing gauze have been published, but none are so good as that just described.

Von Bruns has lately recommended a gauze containing castor-oil instead of paraffin.

His formula is—

Carbolic acid	1 part
Resin	4 parts
Castor-oil	8 ,,
Spirit	20 ,,

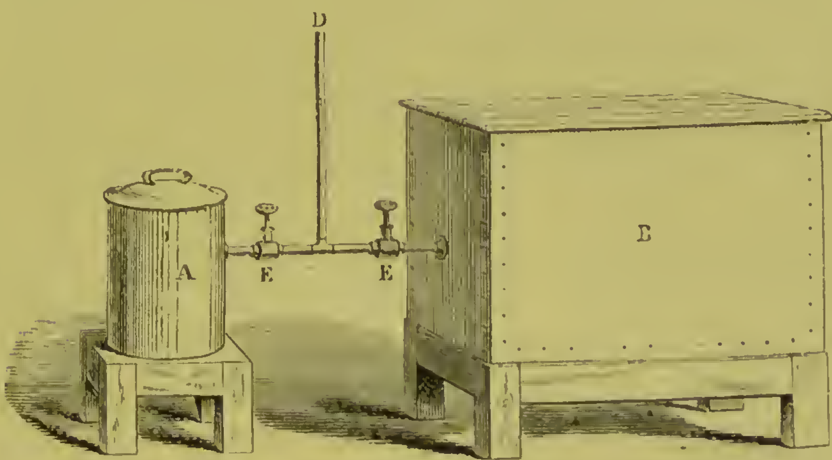

FIG. 21.—MACHINE USED IN THE GLASGOW ROYAL INFIRMARY FOR MANUFACTURING GAUZE.

A is the receptacle for the antiseptic mixture of carbolic acid, resin, and paraffin; B is the tin case in which the muslin is heated, impregnated with the mixture, and submitted to pressure. D is a pipe which conveys steam for heating the apparatus, and E E are stopcocks for turning on or shutting off the steam in connection with either A or B. (From Dr. Beatson's 'Practical Papers on Antiseptic Surgery.')

The resin is first dissolved in the spirit, then the carbolic acid and castor-oil are added. The gauze is simply soaked in this, and then hung up to allow the spirit to evaporate. The objection to this gauze is that the castor-oil interferes with the affinity of resin for carbolic acid, and thus the carbolic acid will be sooner washed out, and the gauze thereby rendered less trustworthy.

In order to prevent the discharge from soaking directly through the dressing *macintosh cloth* is used. This is cotton cloth with a thin layer of india-rubber spread on one

side. It is placed outside the gauze dressing. As a rule one layer of the gauze comes outside it, partly in order to keep it in position, and partly also, in case any hole should exist in the macintosh, to have a little antiseptic material outside. Care is taken that the side on which the india-rubber is spread goes next the wound, for if the other side be placed inwards it absorbs discharge, and, not being itself antiseptic, it becomes in reality a piece of impure cotton in the interior of the dressing, and may thus communicate putrefaction inwards. The macintosh itself also gets spoilt when so used. The dressing consists of a piece of gauze of sufficient size folded in eight layers, beneath the outer layer of which the mackintosh cloth is placed.

Sponges are washed after an operation, and are then kept soaking till required in a jar containing carbolic acid 1–20. During an operation they are washed in 1–40 carbolic acid lotion.

These sponges often become filled with fibrin, and thus rendered more or less useless. It is very difficult to remove this fibrin by washing. Mr. Lister, therefore, after an operation places the sponges in a tank containing water. Putrefaction of the fibrin occurs, and after some days the sponges can be easily cleansed. They are then placed till required in the 1–20 carbolic solution.

When the wound becomes quite superficial, various preparations of Boracic Acid may be employed with advantage.

Boracic lotion is a cold saturated solution of boracic acid ($B_2O_33H_2O$) in water. This acid is soluble in 25 to 30 parts of cold water, and in very much larger proportion when the water is boiling. It is convenient to tinge this solution with litmus in order to distinguish it from the carbolic lotions.

Boracic lint is ordinary surgical lint soaked in a hot saturated solution of boracic acid and then hung up to dry. About half its weight consists of crystals of the acid. This is also stained with litmus.

Boracic ointment is employed in certain cases. Two strengths are commonly used, called full strength and half strength; the former being applied to wounds where cavities exist, the latter to superficial wounds which one wishes to heal

rapidly. The following is the original formula for the full strength ointment:—

Boracic acid crystals	1 part
White wax	1 "
Paraffin	2 parts
Almond-oil	2 "

First mix the wax and paraffin by heating them together, then add the oil; mix the crystals with this in a warm mortar, and continue the process of mixing till the liquid solidifies. Spread on thin cotton cloth.

The half strength contains half the quantity of boracic acid.

A much softer and more manageable boracic ointment is now made with vaseline. The following is the best formula:—

Make a basis of 2 parts of paraffin to 1 part of vaseline.
Take of this 5 parts
" boracic acid, 1 part. Mix.

Salicylic acid cream is used for applying around a wound when a dressing is to be left on for some days. It prevents irritation by the discharge. It formerly consisted of salicylic acid crystals mixed with 1–20 carbolic acid lotion in sufficient quantity to form a creamy fluid. This is apt to separate into two layers, and therefore it is better to make a cream by mixing salicylic acid with glycerine so as to form a paste. This latter cream remains of uniform consistence, and is easily applied. For this purpose Mr. Lister uses glycerine and carbolic acid 1–10.

The formula for *salicylic ointment* is—

Of the same base as is used for boracic acid .	29 parts
Salicylic acid	1 part.

For the purpose of purifying sinuses, putrid ulcers, &c., a solution of *chloride of zinc* is used of the strength of 40 grs. to the oz. of water. This is either applied on lint to the whole surface of a wound, or it is injected by means of a syringe and catheter into all the deep parts of the wound, care being taken to provide free exit for the fluid injected. If the exit of the solution is obstructed, it may pass into the tissues and cause gangrene.

Iodoform is now applied to the surface of ulcers, and

answers the same purpose as the chloride of zinc solution, while it causes no pain.

Carbolised cotton wool is used in some cases of gangrene. It is obtained by soaking pure cotton wool in a 1 per cent. solution of carbolic acid in ether. The cotton is then dried and used immediately.

CHAPTER IV.

ASEPTIC SURGERY (*continued*).

Example of an aseptic operation: Purification of the skin—Fingers—Instruments: Spray—Precautions—Probable errors, and mode of remedying them: Guard: Ligature of arteries: Drainage of wounds—India-rubber tubes—Catgut drains—Horse hair—Decalcified bone tubes (Neuber's and MacEwen's): Sutures: Button stitches—Stitches of relaxation—Stitches of coaptation—Aseptic strapping—Protective: Deep dressing: Loose gauze: Gauze dressing: Elastic bandage. CHANGING THE DRESSINGS—Time—Method. TREATMENT OF ULCERS: Purification of the sore: Boracic dressing: Boracic and salicylic ointment: Boracic poultice.

HAVING described the materials employed in aseptic surgery, we must now consider how they may best be employed. Take as an example of an operation the removal of a fatty tumour.

The patient having been brought under the influence of chloroform or other anæsthetic, the skin over the tumour, and for some distance in the vicinity, is thoroughly purified from any active dust by washing it well with a solution of carbolic acid 1–20. The surgeon and his assistants also wash their hands in 1–40 carbolic lotion, while the instruments are put to soak in 1–20. A towel is arranged close to the tumour, generally on the part of the table between the operator and the patient, which towel has been well soaked in 1–20 carbolic lotion, and is meant as an antiseptic basis on which instruments may be laid during the course of the operation without any fear of their contamination. This towel is so arranged as to be within the cloud of spray. A spray being now made to play over the part from a convenient distance, the surgeon makes his incisions, removes the tumour, ties the vessels with catgut, introduces a suitable drain, stitches up the wound, and applies a piece of protective but little larger than the wound—the protective being of course dipped in the 1–40 carbolic solution.

Outside this is applied a piece of wet gauze, consisting of several layers of loose gauze which has been soaking for some time in the 1–40 carbolic solution. This wet gauze and protective are called the *deep dressing*. The wet gauze must overlap the protective in all directions. Then any remaining hollow is filled up with loose gauze, and outside the whole a gauze dressing is fixed. This dressing consists of a piece of carbolic gauze of suitable size, folded in eight layers, and having the macintosh placed beneath the outermost layer, with the india-rubber side inwards. The dressing is fixed by means of a bandage, and when this is accomplished the spray may be stopped. Then around the edge of the dressing an elastic bandage is applied so as to keep the edge constantly in contact with the body, and to allow no interval to occur between the dressing and the skin during the movements of the patient. The elastic is carefully fixed to the edge of the dressing by means of safety pins.

In the after progress of the case the dressing is changed according to the amount of discharge, though in no instance is it left longer than eight days.

Such is, very briefly, a sketch of the ordinary method of performing operations aseptically. I shall now consider each step in detail, and point out the most frequent sources of failure in carrying out the method; for it must always be borne in mind that the whole operation, as far as regards the avoidance of putrefaction, requires as much care as if it were an experiment performed in a laboratory on putrescible fluid contained in glass vessels.

The first thing, then, is to purify the skin in the neighbourhood of the seat of operation. This is necessary, because the skin is covered with dust. The natural grease of the skin is not easily removed by simple washing, and it protects the septic particles present beneath it and in the hair or sebaceous follicles. This purification of the skin is carried out by washing it well with 1–20 carbolic lotion, the antiseptic being allowed to act for some little time. It is well, having first washed the neighbourhood thoroughly, to apply over the seat of operation a large rag or towel soaked in 1–20 solution, and to allow this to remain on the part for some minutes. Where the epidermis

is thick, or where there is any putrid matter present, it is best to apply this towel about half-an-hour before the operation. It is not necessary to wash the skin with soap and water, or with alcohol or ether, as is often done in Germany. The carbolic acid has a wonderful power of penetrating grease or epidermis; and if time be given for it to act it is unnecessary to wash off the grease beforehand. If the wound is to be in the neighbourhood of hair, as in the axilla or near the pubis, the part must be shaved, and then well soaked with the carbolic lotion.

The errors in the purification of the part may be that the skin is not purified at all, or that it is washed with water; or, as I have seen, the operator simply allows a carbolic spray to play over it for a minute or two, and is satisfied with this; or he merely rubs the surface with his wet finger. This purification must, however, be done thoroughly, for every hair follicle and gland duct may contain causes of putrefaction. Carbolic oil is used by some instead of the watery solution to purify the skin. This is a great mistake, for oil has a much greater affinity for carbolic acid than water has, and therefore the carbolic acid in the oily solution does not act with the same rapidity as the watery solution. Thus 1–20 or even 1–10 carbolic oil is not nearly so useful for producing an instantaneous effect as 1–20 carbolic lotion.

At the same time the operator and his assistants purify their hands. This must also be done thoroughly, and the folds of skin about the nail more especially must be well cleansed with the lotion. In an important operation, as in an operation on a joint, it is well to use 1–20 carbolic lotion for this purpose, so as to avoid any chance of a lurking particle; but in ordinary operations 1–40 is quite sufficient. This purification of the hands is only too apt to be a sham, no care being taken about the nails and folds of skin. The 1–20 is not used in all cases, because 1–40 is really sufficient, and the stronger solution is apt to benumb the hand.

The instruments are purified by immersion in 1–20 carbolic lotion before the operation. A tin or porcelain trough filled with the 1–20 solution is employed for this purpose, the instruments being placed in it some time before an operation (Fig. 22). The instruments are not merely dipped; they must remain in

the lotion for some time, because the carbolic acid requires a little time to act on the grease or dirt on them. For the same reason the teeth of toothed instruments ought to be cleaned thoroughly, and forceps locking by catches ought to be widely opened, so as to allow the solution to come in contact with all parts. The whole instrument must be immersed, for if only the point be purified it may happen that the impure handle is inadvertently brought into contact with the wound during the course of the operation.

The errors most likely to occur are either that during the course of the operation an instrument not previously in the tray is used without any attempt at purification, or that the instrument is imperfectly purified or only part of it cleansed. I have seen the danger of partial purification more than once exem-

FIG. 22.—PORCELAIN TROUGH CONTAINING INSTRUMENTS SOAKING IN CARBOLIC LOTION.

plified. Thus I have seen the blade of a knife alone purified, and the surgeon in the middle of the operation use the unclean handle to separate the tissues. Other errors in the manipulation of instruments will be referred to presently.

The spray is very important in many cases, for it provides an atmosphere in which instruments, &c., may be kept without danger of contamination. In order to have a wide and large antiseptic area in which to work, the spray ought not to be too near, about six or eight feet or more being a suitable distance for a good spray. Care must be taken that the spray is not blown off the part by draughts or by people moving about. The spray is most necessary in opening abscesses or in stitching up wounds, for, to take the latter case, as the wound is not syringed

out after the stitches are inserted, septic air may be inclosed in the cavity of the wound, and may give rise to putrefaction if

Fig. 23.

This figure represents the general arrangement of surgeon, assistants, towels, spray, &c., in an operation performed with complete aseptic precautions. The distance of the spray from the wound, the arrangement of the wet towels, the position of the trough containing the instruments, the position of the small dish with the lotion, the position of the house surgeon and dresser, so that the former always has his hands in the cloud of the spray, and the latter hands the instruments into the spray and various other points, are shown.

the spray has not been playing over the wound while the stitches were being introduced.

During the course of an operation any instrument which has been once purified, if kept in the spray, even though covered with blood, remains pure, and may be introduced into the

wound without hesitation. The same is the case with the hands of the operator or assistants; and therefore the dresser, in handing instruments to the surgeon, *must hand them into the spray* (Fig. 23). If in the course of the operation the surgeon reaches his hand or an instrument out of the spray for any reason whatever, it must be repurified before being put into the wound. For this purpose there is generally a

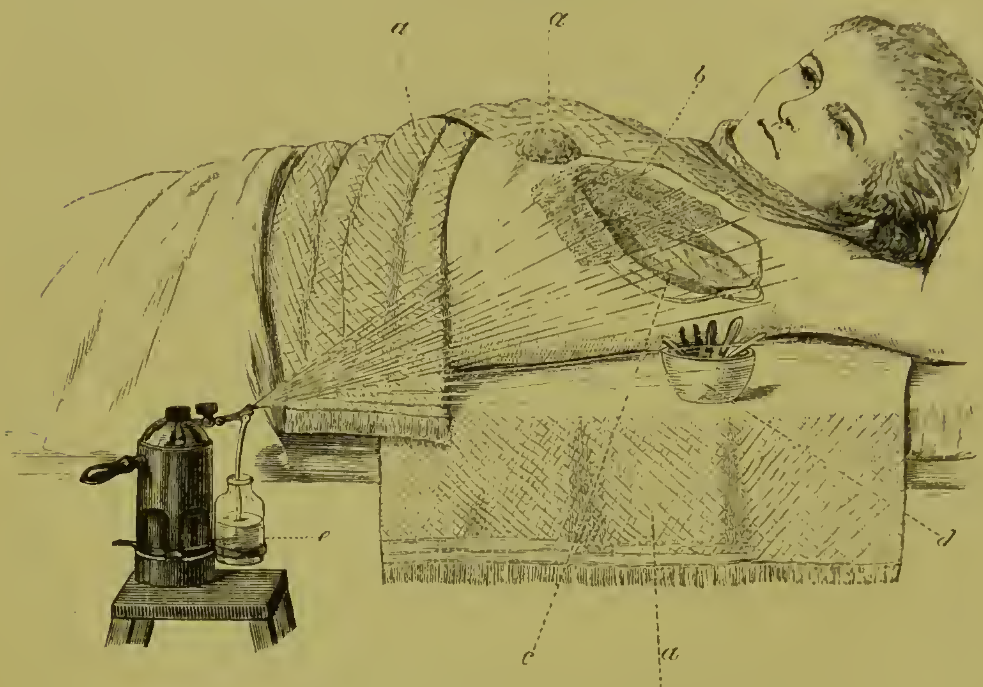

FIG. 24.—TO SHOW THE ARRANGEMENT OF TOWELS, ETC., IN A LARGE OPERATION.

a a a Are three towels which have been soaked in 1–20 carbolic lotion, so that instruments, &c. may be placed on them without fear of contamination. Thus a small sponge will be seen on the upper one. *d* is the dish containing 1–40 carbolic lotion which always stands before the operator, and in the line of the spray. In this he places the instruments which are not being used, and in it he repurifies his hands or instruments if they have been removed from the cloud of spray. In this particular instance we have a large wound to deal with—one so large that a single spray, unless of large volume, may not cover it completely. Hence a piece of *guard* soaked in carbolic lotion is thrown over the front of the wound while the surgeon is attending to the axillary part, or *vice versâ*.

basin of 1–40 carbolic lotion placed close to the operator in the line of the spray, in which fingers, instruments, &c., may be re-purified by momentary dipping. When instruments are laid down out of the spray, or, in the spray, on a blanket, they must be repurified before being used again. To provide a basis on which instruments may be laid, the carbolised towel is arranged before the operator as formerly described, and the blankets in the neighbourhood are generally also covered up

with wet towels, so as to avoid the chance of the instruments being laid on the blankets (see Fig. 24). Should the operator, during the course of an operation, wipe his hands in a dry towel or touch any unpurified substance, he must remember to wash his hands in 1–40 carbolic lotion before re-introducing them into the wound.

These precautions seem on the one hand self-evident, while on the other they seem so burdensome to remember that they are often neglected by self-sufficient surgeons. And yet it is by the neglect of these, rather than by error in any other part of the Listerian method, that mistakes arise and failures occur. Many people think that the spray is the essential part of the treatment, and neglect the precautions as to constant purification of instruments, &c., and when their cases go wrong they say that the principle is incorrect. And yet one thoroughly acquainted with the Listerian method will readily detect the loopholes, and the general loophole is the omission of some of the precautions with regard to purification of fingers, instruments, &c. Thus I have seen a surgeon with considerable experience in aseptic treatment, during the course of a difficult operation wipe his hands with a dry towel and immediately introduce them, covered with the dust from the towel, into the wound. The patient died of septic poisoning. Now many surgeons might have said, 'I used the spray; I used all precautions; my instruments were soaking; my hands were purified;' forgetting this one little incident. When the point was mentioned, however, the mistake was at once seen. People are too apt to trust to the spray as sufficient, and to speak of aseptic or Listerian surgery as treatment by the spray. This is a great and often fatal mistake. Of all the precautions required by Mr. Lister, that of purifying the air by means of a carbolic acid spray is the least necessary, for there are but few septic particles present in the atmosphere, and even though some of them fall on to a wound they may be rendered inert by washing the wound with carbolic lotion. It must always be remembered that Mr. Lister carried out aseptic treatment for years with great success without any spray; and if at the present time he were compelled for any reason to give up some one precaution, he would at once throw aside the spray, as that one which is least

necessary, and which could be the most readily dispensed with. At the same time, the spray is an immense convenience in many cases, more especially in abscesses, empyemata, in stitching up wounds, &c.; and it saves the necessity of applying a great deal of carbolic acid to wounds by irrigating them, with the consequent irritation and risk of carbolic acid poisoning.

To return to the errors which may arise in this part of the treatment. It may be that the spray is too near, and that thus the cloud is so narrow that the surgeon is constantly getting his hands or his instruments out of it, and forgetting to re-purify them. There are other disadvantages when the spray is too near. Thus it is very wetting, and the hands of the surgeon and the wound are unnecessarily irritated by the carbolic acid. If too near, the opaque spray also obscures the field of vision. On the other hand, where the spray is visible, it may be sufficiently trusted. Other sources of error are that instruments may be used which have never been purified, which have been only imperfectly purified, which have after their use lain about outside the spray or on blankets, &c.; or it may be that the carbolic acid gets exhausted in the spray bottle, or that for some other reason the spray does not act properly.

What is to be done should any of these accidents occur? Suppose that an impure instrument or finger be introduced into the wound, that wound must be at once thoroughly washed out with 1-40 carbolic lotion. This is a bad thing for the wound, because it irritates it, and may prevent healing by first intention; or it may, by causing a much larger quantity of discharge than usual, so saturate the gauze dressing as to render it unable to prevent the spread of putrefaction inwards. Therefore it is better to use the spray, and to take all the precautions before mentioned. Should the spray stop, the wound must be washed out just as in the former case, and then, till the spray can be set agoing again, the wound is covered with a piece of rag soaked in carbolic lotion.

This piece of rag, called the guard, ought to be always present in the basin by the side of the surgeon, and when there is any indication that the spray is failing, or should it be advisable to stop the spray for any reason, this is thrown over the wound for the time being. Should any time elapse before the

spray is again ready for use, this guard must be repeatedly moistened with carbolic acid lotion 1–40.

Where the wound is very large it may be protected during the operation either by having two sprays, or by covering up the part of the wound which is not being operated upon by a guard (see Fig. 24).

The arteries are ligatured with catgut. This catgut is generally employed of three different sizes. The largest is used only for large vessels or for stitches; the medium for medium-sized vessels, or for vessels in inflamed or dense tissues where considerable force is required to constrict the vessel, or for stitches; the small or fine catgut is that ordinarily employed

FIG. 25*a*.—METHOD OF TYING VESSELS IN DENSE TISSUES.
(After MacCormac.)

for the smaller vessels. The vessel having been securely tied, the catgut is cut short and gives no more trouble. It is well to tie all the visible bleeding points, because a little oozing of blood may give trouble afterwards from tension. If the vessel be situated in dense tissue, so that a ligature cannot be applied around it, a needle carrying a double catgut thread should be passed through the tissue and tied on each side of the vessel (see Figs. 25*a* and 25*b*). The catgut should be taken direct from the trough containing carbolic oil, and should not be wetted in the lotion. Where the bleeding is from a tear in a large vein, and where it would be dangerous to ligature the

vessel, I have seen the following method adopted by Mr. Lister:—In removing some cancerous glands from the axilla, a small vein was torn away from the axillary vein at their junction, making practically a longitudinal rent in the axillary vein. Taking a fine curved needle and the finest catgut, he stitched up the rent by the glover's suture. The patient recovered without the slightest bad symptom. There was no pain in the wound, nor swelling of the arm, &c. In another case, where the longitudinal sinus was injured in trephining the skull, the wound was plugged with catgut, and the patient recovered without any untoward symptom.

The drainage of an aseptic wound is the point next in importance to keeping the wound aseptic. For if the blood and serum which collect in the interior of the wound within the first twenty-four or forty-eight hours do not get free exit, they give rise to tension, and tension gives rise to inflammation, and the latter, if allowed to go on long enough, to suppuration; and thus the rapid healing of the wound is prevented, though the patient is not as a rule subjected to any danger to life. To avoid these consequences Mr. Lister has paid very special attention to the drainage of wounds. There are two main ways in which this may be done—drainage through tubes, or drainage by capillarity. The former is the most universally applicable and the most certainly successful.

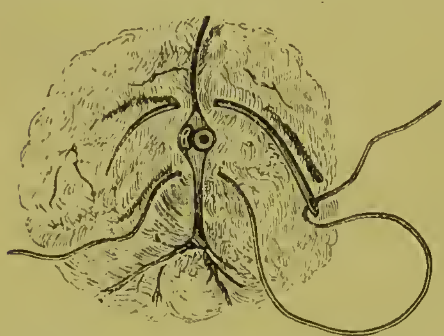

FIG. 25*b*.—ANOTHER METHOD OF TYING VESSELS IN DENSE TISSUES. (From Esmarch.)

Drainage by means of tubes is that first used by Mr. Lister, and, as just stated, is the form of drainage which is most universally applicable. The tubes generally employed are the india-rubber tubes introduced by Chassaignac, though of late the kind of rubber has been altered, that now used being red rubber, which contains no free sulphur. By the use of these red rubber tubes disagreeable smells and blackening of the protective, which often occurred when the black tubes containing free sulphur were employed, are avoided. These tubes have round holes cut in them at short intervals, the diameter of each

hole being about one-third of the circumference of the tube. At the outer end the tubes are cut flush with the surface of the skin—straight across if the tube goes directly downwards, or with varying degrees of obliquity according to the direction

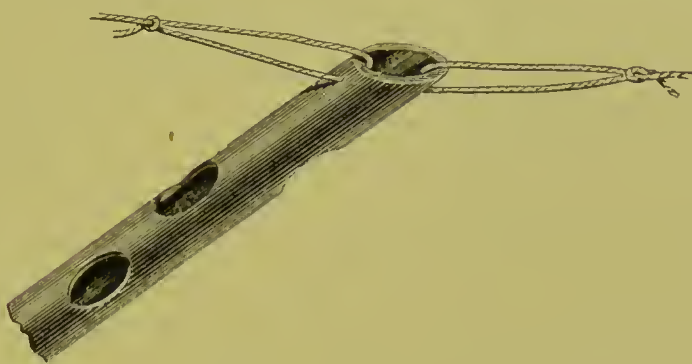

FIG. 26.—ORDINARY OBLIQUE-ENDED DRAINAGE-TUBE READY FOR USE.

which the tube takes (Fig. 26). The tube must not project beyond the surface, for if it does, its orifice gets compressed by the dressing, and the exit of fluid is prevented. To keep the drainage tube from slipping in, two threads of carbolised silk are

FIG. 27.—DRAINAGE-TUBE WITH MASSES OF GAUZE IN THE LOOPS OF THREAD.

fastened into it at its orifice, and tied in a knot. This knot, held between the dressing and the skin, retains the tube in position. In some cases, however—as for example, in empyema—the tube might slip in in spite of these threads, and therefore it is well to fill up the loops with strips of gauze soaked in the

carbolic lotion (Fig. 27). These absolutely prevent the tubes from slipping in. These tubes are always kept in a large vessel containing 1–20 carbolic acid solution, and are thus always ready for use. When a tube is altogether removed from a wound it is not thrown away, but is washed and put into the bottle with the other tubes, and used for another case. These tubes vary in size according to the size of the wound and the amount of discharge expected, and are arranged so as to drain the parts of the wound which form cavities or from which the greatest amount of discharge will come It is not necessary that their orifices be dependent, though it is of course better that they should be so. It is not essential, however, because the fluid, as it forms, wells out, and, not being putrid, that which lies at the bottom of the drainage-tube does not cause irritation. In cases where the most dependent opening would be near sources of putrefaction, it is well to have the drainage-tube in another part of the wound, even though it be not so dependent. Thus in inguinal hernia the tube would no doubt be in the most dependent part if its orifice were close to the pubis, but as that would be much too near sources of putrefaction, such as the vagina and penis, the orifice of the tube ought to be at the outer angle of the wound (see Fig. 28). In a large wound it is well to have more than one tube; and it is better to have two smallish tubes in any case, rather than one large one, because on the day after the operation one of these tubes may be removed altogether; whereas if a large one were pulled out in order to insert a smaller, there would be the greatest difficulty in introducing either. No tube which one wishes to put back again should be removed till the third

FIG. 28.—INCISION FOR INGUINAL HERNIA, STITCHED, SHOWING THE POSITION OF THE DRAINAGE-TUBE AT THE OUTER ANGLE OF THE WOUND.

day, on account of the difficulty of returning it. By that time, however, it lies in a channel in the blood clot or lymph, and slips back easily. Fig. 29 represents forceps introduced by Mr. Lister, and called 'sinus forceps,' which are of the greatest service in inserting drainage tubes. Generally on the third day half the tube is cut off, and it is reduced in length at subsequent dressings till it becomes no longer necessary. No exact rules can be given for shortening or leaving out the tube. This must simply be a matter of experience, guided by the amount of discharge and the tendency to accumulation or otherwise. Should tension occur, a larger and longer tube ought to be at once introduced.

A point which has always seemed to me of great importance in connection with the use of these tubes, and one which has apparently been overlooked, is the following. A tube is taken

FIG. 29.—SINUS FORCEPS.

out of carbolic lotion at some distance from the spray, is carried through the air, and then directly introduced into the wound. I can hardly believe that when a large tube is taken out of the lotion there would be sufficient vapour of carbolic acid in it to destroy any septic dust which might get into its interior, for a considerable mass of air must take the place of the fluid, and this amount of hospital air may often, as I have found by experiment, contain causes of putrefaction. Of course when passing through the spray this air may be displaced or purified, and also when introduced into the wound a considerable amount of it would be forced out; while at the same time there is a good deal of carbolic acid present, and purification in one way or another would probably occur. And further the purifying power of healthy living tissues, which will be afterwards demonstrated, must be taken into account. But in the case of a cavity, purification in any of these ways may

not happen, and putrefaction may result. In a case of incision into the knee-joint, which will be afterwards alluded to, in which fermentation and inflammation occurred, this seemed to me the most probable explanation. My suggestion therefore is always to take the tubes out of the lotion *in* the spray, and then the air which enters them will be air previously acted on by the spray.

Drainage by capillarity was introduced by Mr. John Chiene, who was also the first to enunciate the principle of absorbable drains. For this purpose he uses catgut, and generally the

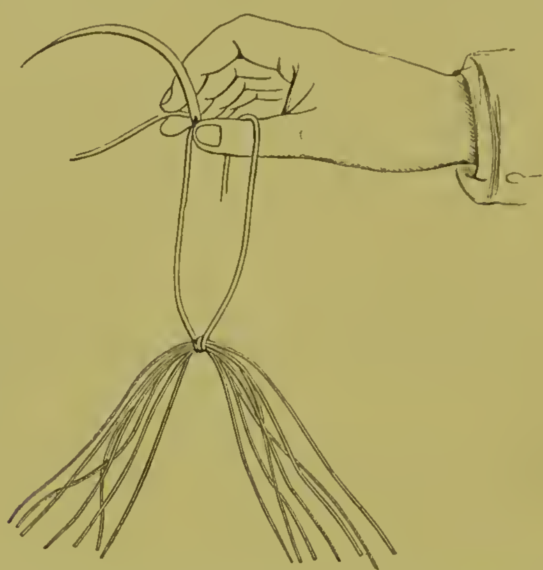

FIG. 30.—CATGUT DRAIN READY FOR INSERTION.

finest threads. A skein of catgut, containing say twenty threads, is tied at its middle by a single thread of the same gut. One end of this thread is passed through a needle (Fig. 30), and by means of this the centre of the skein is stitched to the deepest part of the wound (Fig. 31). The skein is now broken up into bundles of five or six threads each. One bundle comes out at each angle of the incision, and the other bundles at intervals between the stitches (Fig. 32). More than one skein may be required in a large wound. This catgut becomes absorbed, and never requires to be removed. In five or six days the ends which hang out drop off, and little granulating sores are formed which heal in a few days. In this method the serum escapes by capillarity, and by distributing the threads

over various parts of the wound the true principle of drainage is carried out; for, as pointed out by Mr. Chiene, in draining a field one does not have one large drain going from one end of the field to another; on the contrary, the field is traversed by numerous small drains. And so in Chiene's method of draining wounds we have a number of small drains traversing the wound in several directions. In this method there is no trouble about pulling out the drain, and no necessity for changing the dressing simply to remove a tube; the drain disappears of itself. It is well to leave the ends of the catgut outside the wound as long as possible, so as to get a siphon action, and care must be taken not to break up the bunches of catgut outside the wound, for the capillary action occurs in the intervals between the threads when they are closely apposed.

The objections urged against this method are, firstly, that in large wounds it is not sufficient, and that the catgut becomes a pulpy mass, and when in large quantity takes a long time to organise. Not only may it be insufficient at first, but it may become absorbed too soon—before, indeed, a drain of some kind can be dispensed with.

Now these objections rest in great part on the fact that the drain is often improperly employed. If, for instance, it be not stitched to the deepest part of the wound, the catgut may slip and the deeper parts may not be drained; and again, if a large bundle of it be used, coming out at one part of the wound only, it does become a pulpy mass, and takes a long time to organise. But this is not the method recommended by Mr. Chiene, for he says that only five or six threads ought to be brought out at each place. There is, however, no doubt that in some cases it is absorbed too quickly, and this was the real objection to the use of this method when we had only the catgut prepared by the old method, though even with it, if the gut was well prepared and old, the drainage was often very satisfactory. This difficulty will probably be overcome by the use of the chromic catgut recently introduced; the only fear, indeed, will now be that the drain may last too long. Mr. Lister, however, thinks that if only the finest gut be used, according to Mr. Chiene's directions, the probability is that it

will be absorbed with sufficient rapidity. Of course, if necessary, the ends of the drain can be cut off, when it has served its purpose, below the level of the skin, and then, even though the internal part be not all absorbed, the wound can heal completely.

Catgut can only drain fluids such as blood or serum; it cannot drain pus. It is, however, unsuitable in cases of chronic abscess, where we have only a serous discharge, because the catgut is absorbed long before a drain can be dispensed with.

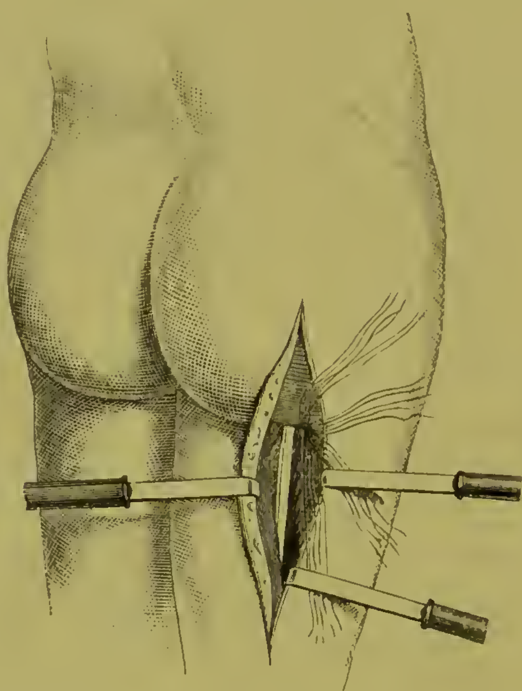

Fig. 31.—Operation for stretching the sciatic nerve.

Catgut drain stitched to the deepest part of the wound, beneath the gluteus maximus, and broken up into four separate bunches.

If the wound is very large it is well to introduce tubes as well as catgut drains at first. The tubes may be removed in twenty-four hours.

Instead of catgut, horse-hair has been a good deal used. This is simply laid into the wound in the situation where it seems most required. It is diminished by degrees, threads being taken out at various intervals of time. It has an advantage over catgut in draining joints, for no part of the drain

remains in the interior of the joint, while portions of catgut do. Further, it is not absorbable.

But it has the same disadvantages as the drainage tubes, and it is not readily retained in the deeper parts of the wound. It is preferred by Mr. Lister to the catgut, but there can be no doubt that the catgut, when used strictly according to Mr. Chiene's directions, and of good quality, is a very efficient method of drainage.

It is easy to re-introduce a horse-hair drain if necessary by proceeding in the following manner:—A sufficient quantity

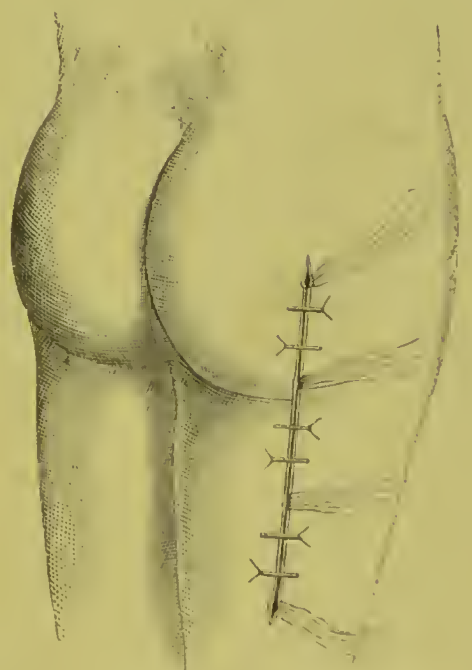

FIG. 32.—THE SAME WOUND STITCHED.

The bunches of catgut coming out at intervals between the stitches. (The wound has been exaggerated, and the threads of catgut separated, in order to show the method more clearly. The threads of catgut ought to lie in close apposition, for it is the intervals between the threads which act as capillary drains.

being taken, the bundle is bent at its middle over a probe, and tied close to the probe by carbolised silk (Fig. 33). In this way, the probe being withdrawn, a blunt compact end is obtained, which may be introduced into the wound with ease.

Of late the principle of absorbable drains has been applied by Dr. Neuber of Kiel [1] in his absorbable drainage tubes.

[1] *Ein Antiseptischer Dauerverband nach gründlicher Blutstillung. Von Langenbeck's Archiv*, Bd. xxiv. Heft. 2.

These are tubes drilled in long bones, and then decalcified and carbolised. Holes are afterwards cut in the sides, and they are used like ordinary india-rubber tubes. These tubes are said to answer very well, though they are sometimes absorbed too soon, and sometimes last too long. They sometimes get soft and collapse about the third or fourth day, and thus, though not absorbed, become useless as a drain.

Dr. MacEwen [1] has lately somewhat modified Neuber's tubes. He uses chicken-bones, which are already hollow, and decalcifies them. 'The method of preparation is as follows :—The tibiæ and femora are scraped and steeped in hydrochloric acid and water (1 to 5) until they are soft. Their articular extremities are then snipped off with a pair of scissors ; the endosteum is

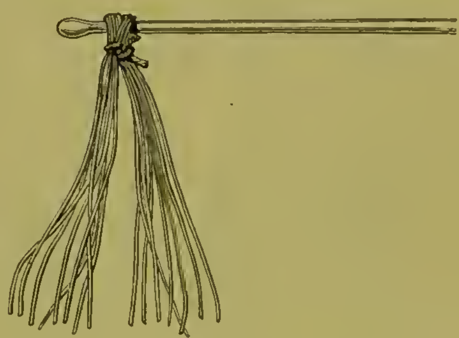

FIG. 33.—METHOD OF PREPARING A HORSE-HAIR DRAIN FOR RE-INTRODUCTION.

raised at one end and pushed through to the other extremity, along with its contents. They are then re-introduced into a fresh solution of the same strength until they are rendered a little more pliable and softer than what is ultimately required (as they afterwards harden a little by steeping in the carbolised solution). When thus prepared they are placed in a solution of carbolic acid in glycerine 1–10. They may be used at the end of a fortnight from the time of introduction into the glycerine solution. Holes may be drilled in them with a punch, or clipped out with scissors.' These tubes are threaded with horse-hair before being introduced into the tissues. This hair helps to maintain the calibre of the tube during the first few days, and also itself acts by capillarity.

[1] *British Medical Journal*, Feb. 5, 1881.

The average duration of MacEwen's tubes in the tissues was something over eight days. If, however, a tube is likely to be required for a longer time, it can be obtained by steeping the decalcified tubes in a chromicised instead of a carbolised solution. These resist the action of the tissues for two or three weeks.

The accurate stitching of the edges of the wound is another feature in aseptic surgery. In operating aseptically the same care need not be taken to remove as little skin as possible as is necessary in wounds treated by other methods where swelling and inflammation of the edges are expected. One may take away a wide sweep of skin, such as would seem to render hopeless any attempt to bring the edges of the wound into apposition; and yet if the edges can only be apposed, and if

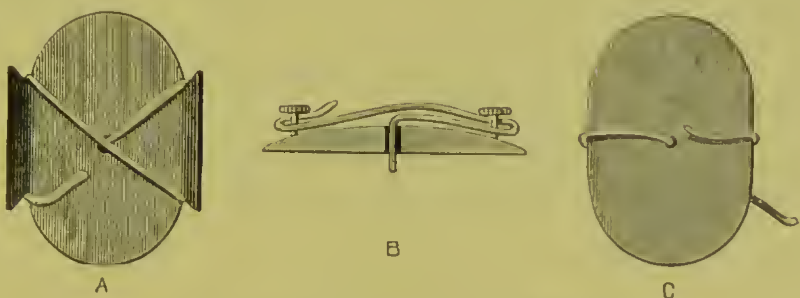

FIG. 34.—LEAD BUTTONS FOR DEEP STITCHES.

A, The present form, described in the text. B, Form of button devised by Dr. Ogilvie Will (seen in section) C, The old form, where the wire was fastened by passing it beneath the button.

the wound remains aseptic, union by first intention may be expected along the whole line.

Button stitches are employed to relax the edges of the wound, and thus to leave the cutaneous margins free from the irritation which must occur if they are tightly drawn together. These consist of flat pieces of lead cut of an oval form and of various sizes, perforated in the centre by a hole through which silver wire is passed, and provided with two lateral wings round which the wire is twisted (Fig. 34). (There are various forms of button suture, but all act on the same principle.) These are applied some distance on each side of the edge of the wound, and connected by strong silver wire drawn tight enough to permit the edges of the wound to come pretty easily together. The number used varies according to the amount of tension.

In order to bring the edges of the wound into actual contact, two sets of stitches are employed: silver wire stitches, which take a good hold of the tissues and are placed at regular intervals, termed stitches of relaxation; and in the intervals between these, in order to have the cutaneous margins accurately applied to each other, numerous stitches of coaptation, consisting of carbolised silk, horse-hair, or catgut are inserted (Fig. 35). The speedy healing which occurs when the edges of the wound are accurately brought in contact, while they are at the same time, by the button stitches and the stitches of relaxation,

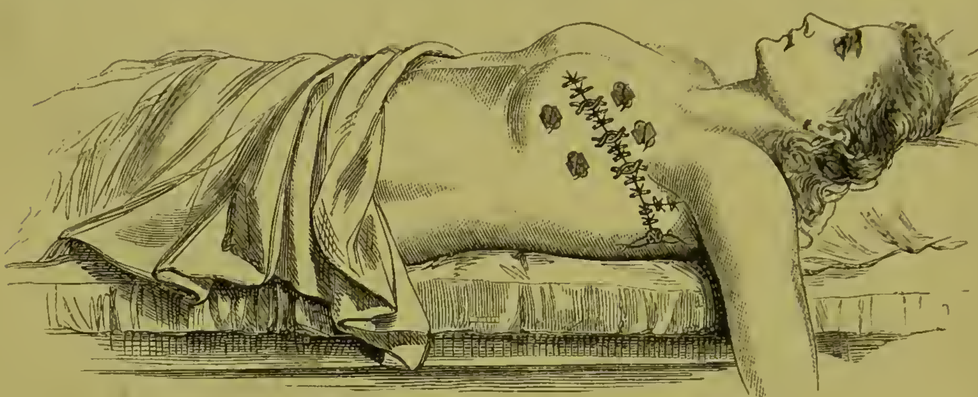

FIG. 35.—WOUND AFTER REMOVAL OF MAMMA AND AXILLARY GLANDS, STITCHED.

To show the three kinds of stitches. The button stitches will be at once recognised; the thick stitches, of which three are represented, are the stitches of relaxation; and the remainder are the stitches of coaptation.

freed from any tension, rewards the surgeon for the time spent in inserting a large number of these stitches of coaptation.

In taking out these stitches it is best to follow a reverse order to that of insertion. The first to be removed are the stitches of coaptation, while the stitches of relaxation are probably cut on the same day. Do not be in a hurry to remove the stitches where there was much tension in bringing the edges of the wound together. A week or ten days is time enough.

Should the wound gape, strapping may be employed, even under an antiseptic dressing. To render the strapping aseptic, it is immersed in warm carbolic lotion (one part of 1–20 and an equal part of boiling water) before being applied. This both

renders it aseptic and also takes the place of the hot-water can for heating the strapping.

Having proceeded thus far in the aseptic operation -- having tied the vessels, arranged the drainage, and brought the edges of the skin well together— we must now apply a dressing which shall prevent the occurrence of putrefaction till the case is again seen.

In applying a dressing we must in the first place be careful to make it as little irritating as possible to the young epithelium along the line of incision. The dressing employed is the carbolic gauze; and, to prevent the irritation of the healing edge of the wound by the carbolic acid, a piece of protective is interposed between the gauze and the wound. This protective is cut a little larger than the wound, and it is well to cover the buttons with a little bit also, in order to prevent the threads of the gauze from becoming entangled in them. This protective need not extend over the orifice of the drainage tube, as its essential object is to protect the healing part from the irritation of the carbolic acid. The protective is also of use in preventing the dressing from sticking to the wound, and in preventing the formation of scabs, and the consequent possible retention of the discharge.

An error which is frequently made is to put on too large a piece of protective. There is nothing antiseptic in its substance, and it protects the discharge beneath it from the action of the carbolic acid. Therefore if at any part it projects beyond or comes close to the edge of the dressing, it allows the causes of putrefaction to spread inwards beneath it, and prevents the carbolic acid from acting on this putrefying discharge. It is therefore a very good rule, having covered the wound with sufficient protective, to look on this protective as a wound, and to be as careful in having the gauze dressing overlap it in all directions as if it itself were the wounded surface. Where there is very little space for overlapping, as in inguinal hernia, no protective ought to be applied. It is better to have somewhat slower healing than to have putrefaction spread into the wound. As mentioned before, this protective is dipped in carbolic lotion 1–40 before being applied.

Outside the protective a piece of gauze wet in the carbolic

lotion 1–40 is applied so as to overlap the protective in all directions. The reason for this is that dry gauze is apt to receive dust on its surface before being used, while at the ordinary temperature of the atmosphere but little carbolic acid is given off from the gauze, certainly not enough to destroy immediately the activity of the septic particles in the dust. But if the piece of gauze applied next to the protective be moistened in the 1–40 solution, this dust is at once deprived of septic energy, and we apply over the wound a layer of pure and powerfully antiseptic material.

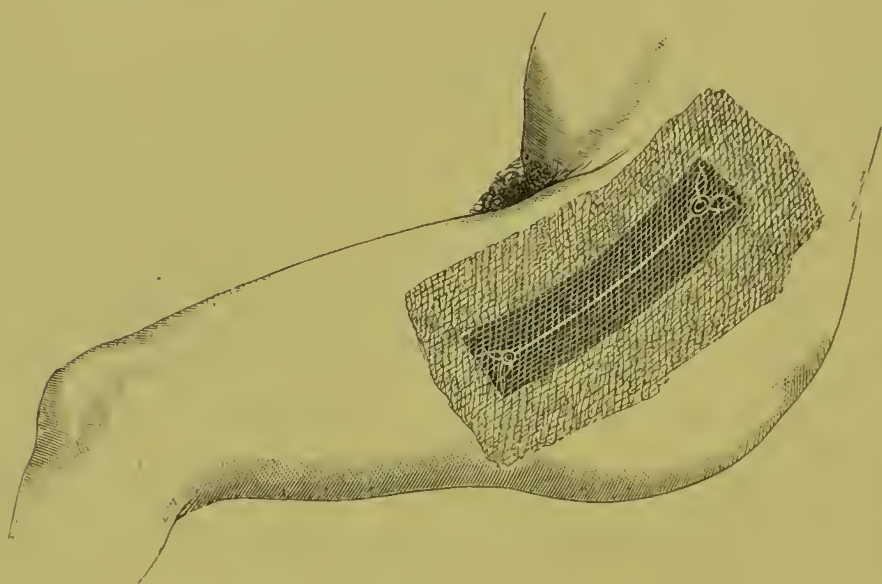

FIG. 36.—EXCISION OF THE HIP-JOINT.

Wound stitched; protective and deep dressing applied.

The piece of wet gauze and the protective go by the name of the deep dressing. This deep dressing may in some cases, and more especially where catgut stitches and catgut drains are used, be left for several days undisturbed. In this way the wound is not irritated by the application of carbolic acid to it every time the dressing is changed. If the deep dressing be thus left on, it must be remembered that the deep piece of gauze loses all its carbolic acid very soon, and that therefore it must be treated as a wound—*i.e.*, in renewing the dressing this deep part must be overlapped in all directions by a piece of wet gauze, and that again by a dressing of suitable size.

In some cases it may be desirable to fix down the deep dressing with a piece of gauze bandage. If it be intended to leave on this deep dressing for some time it is well, before applying it, to rub the neighbourhood of the wound with the salicylic cream mentioned before. It sometimes happens that when a dressing is left on for many days together, the discharge becomes somewhat irritating, and the skin around the wound becomes excoriated. This is generally entirely prevented by the use of salicylic cream.

Having arranged the deep dressing in a suitable manner, any hollows which exist in the neighbourhood of the wound are filled up with carbolic gauze, and special masses of this material are placed where the greatest amount of discharge is expected. Outside this a large gauze dressing, made as before described, is applied. The size of this dressing varies according to the amount of discharge expected, but in all cases it must extend well beyond the deep dressing in all directions. Some special examples will be mentioned presently.

This dressing is fixed on with a suitable bandage. The gauze bandage is preferable to an ordinary bandage under certain circumstances. It is especially convenient in bandaging a stump next the skin to prevent retraction of the flaps, and also for fixing down the deep dressing. It also increases the amount of antiseptic material outside the macintosh if there happens to be a hole in it. But for ordinary use in fixing on dressings very light and cheap bandages may be made from the ordinary thin muslin which is used as a guard. They do not stick to the skin as the gauze bandage is apt to do.

The dressing is pinned round its edge to the bandage. Care must be taken not to put pins through the macintosh at any part except at its edge. Pinholes through the centre of the macintosh simply defeat its object by permitting the discharge to come directly through the dressing. The object of the macintosh is to make the discharge travel through a large extent of the gauze, and thus the same result is obtained as if a mass of gauze were applied over the wound, of the same thickness as the distance from the centre of the macintosh to its edge. If therefore there be a pinhole near the centre of the macintosh, the object of the latter is seriously interfered

with. Accordingly, it is always the duty of the person who makes the dressings to examine the macintosh with the view of detecting any holes in it.

If the dressing is to be used as soon as it is made up, it is well to sponge the surface of the macintosh with 1–20 carbolic lotion before inserting it. The same piece of macintosh may be used for a whole case, or for more than one—so long, in fact, as it does not become worn into holes. Two pieces of macintosh are generally provided for each case, and a dressing is always made immediately after the case has been dressed, and is ready for application at any time.

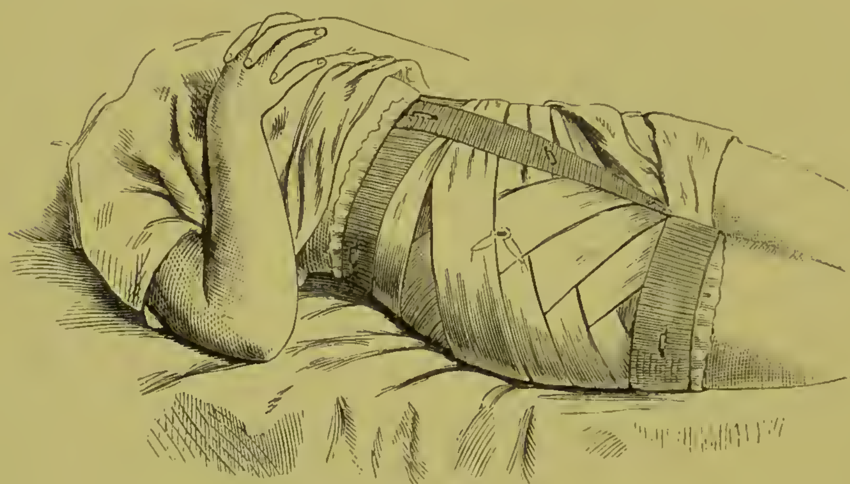

FIG. 37.—DRESSING IN A CASE OF PSOAS ABSCESS OPENED ABOVE POUPART'S LIGAMENT.

To show the arrangement of the elastic bandage along the margins of the dressing.

It might happen that, in the movements of the patient, the edge of the dressing might become separated from the skin, and air pass into the space thus formed. To prevent this, the German surgeons as a rule pack in salicylic jute or wool beneath the edge of the dressing. This may serve the purpose, but it is by no means safe. Mr. Lister some time ago introduced the use of elastic webbing, which is of various breadths. It is better not to be too broad. It is put moderately on the stretch, and surrounds the edge of the dressing. Its general arrangement varies of course with the situation. It is not much used on the extremities, because the arm or leg is generally so

fixed by means of splints that there is no chance of separation of the dressing.

The operation and first dressing having now been completed, the question arises when the dressing should be changed. It is only extremely rarely that it is necessary to change it the same evening. The only cases in which this is usually done are large empyemata or very large abscesses, and cases of amputation at the hip-joint, where the discharge of bloodly serum is profuse, and there is but little space for overlapping of the dressing.

As a rule, the dressing ought to be changed entirely on the following day, the deep part as well as the superficial. It is well to change the deep dressing in order to see that none of the stitches are too tight, and that the drains are acting properly. After the first day the deep dressing need not be touched, unless the patient is complaining of uneasiness, or unless the surgeon wishes to see the wound for the purpose of removing stitches or drain. If it is not necessary to disturb it, it may, especially where there is an organising blood-clot, be better not to do so, for that would only be to expose the wound unnecessarily to the irritation of the carbolic acid. If the deep dressing is not changed, great care must be taken to have an efficient spray playing over the part.

In changing the dressing the spray is used, and also 1–40 carbolic lotion, in which a piece of loose gauze and protective are put before the dressing is begun. The elastic bandage is first removed, and then the patient or an assistant places his hand over the centre of the dressing while the bandage is being cut, so as to prevent the dressing being lifted up and air pumped in. Then the surgeon, having purified his fingers, and having turned on the spray, lifts the edge of the dressing carefully, taking care that the spray passes into the angle between the dressing and the skin (see Fig. 38). Having removed the superficial dressing, he again dips his fingers, and then removes the deeper parts and exposes the wound.

If nothing is wrong, he immediately applies fresh protective and wet gauze, and then washes the parts round about, as far as the discharge has extended, with 1–40 carbolic lotion. The

edge of the wound is not washed or exposed to the action of the spray longer than is absolutely necessary. It is well to apply the deep dressing at once, for in washing the surrounding parts, one is apt to give the wound a final touch with the rag. Now this rag may contain some gross particles of putrid material (such as a crust of discharge from the exterior of the dressing, fæces, &c.), and thus putrefaction would be communicated to the wound. There is no necessity for cleansing the edges of the wound. Dirt, so long as it is clean, *i.e.*, so long as it does not contain causes of putrefaction, does no harm; indeed, it rather aids the action of the protective; while

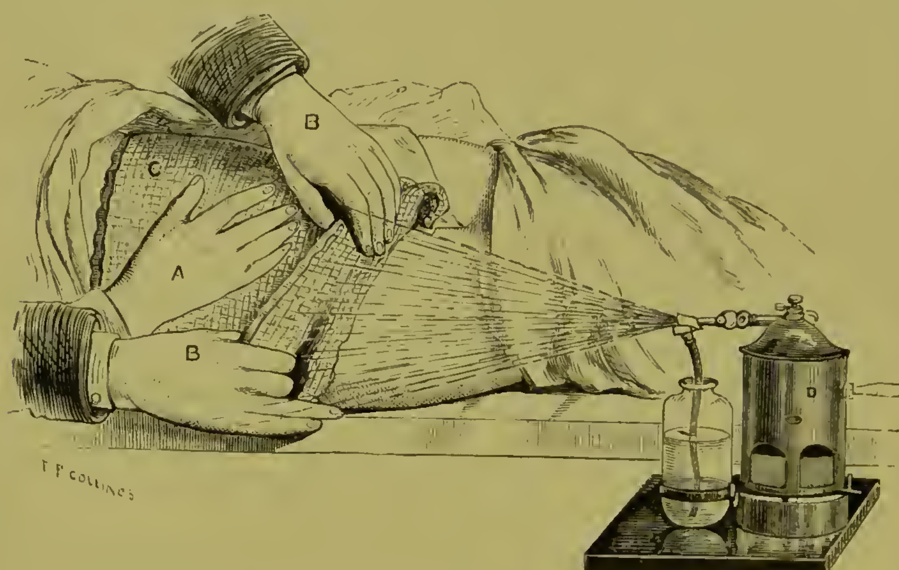

FIG. 38.—METHOD OF CHANGING A PSOAS ABSCESS DRESSING.

A, Hand of patient holding down the dressing over the wound. B B, Hands of surgeon lifting the lower edge of the dressing (C). D, The spray machine so placed that the spray passes in beneath the dressing as it is lifted.

to rub it away is to irritate and injure the healing edge—to produce a state of unrest. A fresh dressing is applied as before described.

Where there are two wounds in different situations, so placed that the spray cannot command both, each must be dressed separately, care being taken not to uncover the one till the other is at any rate protected by a deep dressing. The patient or assistant must keep his hand on the dressing over the one wound, while the other wound is being attended to.

The next dressing takes place on the following day at visit,

if there is any discharge at the edge of the dressing or if the wound feels uneasy. If there is no discharge on the drawsheet and if the wound is free from pain, the dressing is not changed; and even though discharge should appear a few hours later, the dressing is not changed till next day at visit hour. The rule for changing the dressings is therefore: Change if discharge is through at the visit hour, or if there be any other reason for it; if not, leave the dressing till next day at visit, and then follow the same rule.

Never leave a dressing unchanged longer than a week. By that time most of the carbolic acid has passed off by evaporation; and therefore, if the discharge once came to the edge, putrefaction could spread inwards with great rapidity. And it would not be necessary for the discharge to appear at the edge in order to have putrefaction of the wound, for the sweat collecting beneath the dressing permits the multiplication of septic particles in it, and thus they may reach the wound. Where a dressing is to be left on for a week, it is well to use the salicylic cream in the way before described.

Such is the general method of using carbolic dressings; special modifications will be noticed presently. Let me pass on in the meantime to the general points as to boracic dressings.

Let us suppose that a patient is admitted with a foul ulcer of the leg: how is he to be treated? If he were to be treated with carbolic dressings, the ulcer would very probably remain foul, or even though it ultimately became free from odour, it would heal excessively slowly. Hence, Mr. Lister first purifies the sore, and then dresses it with boracic acid.

To purify the sore, chloride of zinc, 40 grs. to the ounce of water, may be used. This is applied thoroughly to the whole surface of the sore, and at the same time the surrounding skin is well purified by thoroughly washing it with 1–20 carbolic lotion, which is employed on account of its special power of penetrating the epidermis. When this has been done, a piece of protective, dipped in boracic lotion and slightly larger than the sore, is applied over it, and outside this is placed one or two layers of moist or dry (it does not much matter which)

boracic lint, of sufficient size to cover the protective well in all directions. There is the same objection here to allowing the protective to project beyond the edge of the dressing as in the case of the carbolic dressings. Lately, instead of applying the chloride of zinc solution, which causes considerable uneasiness, iodoform has been powdered over the whole surface of the ulcer, and it has been equally successful. The chloride of zinc or the iodoform need only be applied once; but should putrefaction not be eradicated, the application is repeated.

This dressing is changed next day, but afterwards, as a rule, it only requires to be changed every two or three days, or indeed at longer intervals, provided that there is not much discharge. That is to say, as there is a very large store of the antiseptic in the lint, and as it is but slightly soluble at the temperature of the human body, the discharge may go through the dressing many times without washing out all the antiseptic. At the same time it is found as a general rule that the wound heals most rapidly when the dressing is changed once in three or four days.

At the changing of the dressing no spray is required. The bandage (which may be a common cotton bandage, if preferred) having been removed, the dressing is taken off and the wound well washed with boracic lotion. Any septic dust which falls on the wound during its exposure is destroyed by giving the wound a final wash with the lotion before applying a fresh piece of protective and boracic lint.

This boracic dressing is not used for wounds which are not quite superficial, because the acid is not volatile, and because it is but a feeble antiseptic; but when once a wound has become quite superficial, it will heal more quickly if treated with boracic dressing.

In some cases, more especially where the sore is septic, or where the patient dresses it himself, boracic ointment is preferable to protective, and where the sore is healing, the half-strength ointment is the best. Outside the ointment a piece of boracic lint is applied as usual. Of late, salicylic ointment has been used, and found to answer, as a rule, better than the boracic. It is less irritating, and permits healing more readily.

A eucalyptus ointment has been employed quite recently, and has given excellent results.

When the effects of a poultice are wanted along with an antiseptic effect, the boracic lint is applied like water dressing. A suitable piece of the lint moistened in boracic lotion is applied, and outside this a larger piece of macintosh or gutta-percha, overlapping the lint in all directions.

CHAPTER V.

ASEPTIC SURGERY—(*continued*).

Special dressings: *Head dressings: Neck dressings: Breast dressings—Abscess of mamma—Excision of mamma alone—Excision of mamma and axillary glands: Axillary dressings: Dressings on the limbs: Dressings for psoas abscess: Lumbar abscess: Hip-joint abscess: Dressings in cases of hernia and operations on the scrotum: Excisions of joints.* Aseptic treatment of abscesses. *Chief points to be considered in opening abscesses—Method of opening abscesses—Drainage of abscesses—After-treatment of abscesses—Empyema—Perineal and anal abscesses.* Treatment of wounds produced accidentally: *Problem to be solved—Purification of wound—Further treatment of the wound.* Special wounds: *Compound fractures: Wounds involving tendons, nerves, &c.: Wounds of joints: Compound fractures of the skull: Penetrating wounds of the thorax: Wounds of the abdomen.* Putrid sinuses and wounds. Treatment of burns. Treatment of gangrene. Treatment of nævi and varicose veins.

I SHALL now describe the special methods of dressing and other precautions required in different situations.

In operating on the *scalp* the hair must be shaved for some distance around the wound, and the hair beyond ought to be soaked with carbolic lotion 1–20. If the incision be in the centre of the scalp, or, in other words, if there be a circle of hair all round it, it is better not to use protective at all. The dressing in such a wound is fixed by the ordinary capelline bandage. Where the wound is more or less to one side, the dressing must extend downwards on the neck, and it is then well to have a narrow elastic bandage along the edges, more especially around the neck. In the neighbourhood of the ears, the various cavities in the ear, and the space behind it, must be filled up with gauze.

Neck dressings have nothing very unusual about them. The dressing must be fastened round the neck. It must be prevented from slipping down by a turn passing above the ears

and around the forehead, and also by two vertical turns over the head, one transverse and the other longitudinal, these various turns being pinned together where they cross. To prevent the dressing from slipping up, turns are passed under the axilla. A narrow elastic bandage must be applied round the edge of the dressing in this situation, for the movements of the head are extremely apt to cause an interval between the skin and the dressing (Figs. 39 and 40).

FIG. 39.

This figure illustrates the general arrangement of dressings on the neck. The arrangement shown here would do for any operation about the region of the sternomastoid behind or below the ear.

Breast dressings are very important; they are arranged in three different ways according to the size and extent of the wound.

No. 1.—Where an abscess of the mamma is opened, or where some small incision, not interfering with the form of the organ, is made, the dressing consists of an ordinary gauze dressing covering the whole mamma, some loose gauze being packed in in front and behind. This is fixed by turns of bandage passing round the body alternately above and below the organ, with straps over the shoulder. The arm is placed in a sling. The edges are fixed by elastic bandage (Fig. 41).

No. 2.—Where the mamma has been removed and the discharge has become much diminished in amount, there may remain enough of room between the wound and the axilla for overlapping of the dressing. In order to fix the dressing and keep it well up in the armpit, it is split vertically at the axilla, folded over, and pinned on the top of the shoulder. It is then bandaged securely, and an elastic bandage applied around the edges (Fig. 42).

No. 3.—Where the mamma and axillary glands have been removed, or for the first few days after excision of the mamma alone, this arrangement is not enough, for it does not leave sufficient room for overlapping. The arm must therefore be

included in the dressing. This is accomplished most conveniently in the following manner:—A large dressing is applied

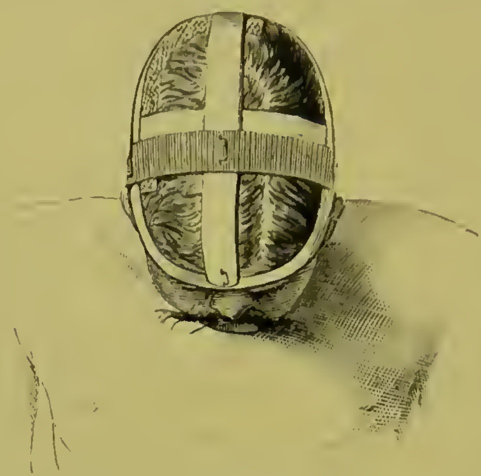

FIG. 40.—TO SHOW THE ARRANGEMENT OF THE TURNS OF BANDAGE ON THE HEAD SEEN FROM ABOVE.

posteriorly, reaching behind as far back as the middle line, and folding over the arm so as to touch the thorax in front,

FIG. 41.—DRESSING APPLIED IN A CASE OF ABSCESS OF THE MAMMA (BREAST DRESSING NO. 1).
The position of the drainage tube is indicated by dotted lines.

FIG. 42.—BREAST DRESSING NO. 2.

the arm being applied to the side. This dressing must be broader than the length of the upper arm from the top of

the shoulder to the tip of the elbow, the overlapping parts being caught by the turns of bandage passing over the shoulder and round the body respectively. To prevent the internal condyle from suffering from the pressure, a large mass of gauze is applied behind the arm, extending downwards almost to the condyloid region, but not reaching quite so far. A mass of gauze is packed in between the arm and the side, and in front filling up the angle between the arm and the thorax (Fig. 43). A smaller anterior dressing is then applied, narrower than the posterior, reaching as far forwards as the middle line or beyond it, and outwards to the upper arm, the edge of the anterior dressing passing beneath the edge of the

FIG. 43.—CASE OF EXCISION OF THE MAMMA.

Back dressing lying ready for application; showing also the deep dressing and padding in the axilla and behind the arm.

posterior. Thus the side of the patient is completely encased in a gauze dressing. This is very easily bandaged on. One turn of bandage passes round the body outside the arm (Fig. 44, 1); the second also passes round the body, but below the elbow (2), thus catching the portion of the dressing overhanging the elbow and also the lower edge of the front dressing; the next passes round the body and over the top of the shoulder on the side operated on, thus catching the portion of the dressing projecting above the shoulder (3); the bandage then passes down behind but parallel to the arm, turns round below the elbow, runs obliquely upwards in front to the top of

the opposite shoulder (4), then obliquely back again behind the body (thus fixing the upper angles of the dressing in front and behind) to the middle of the arm, over which it passes obliquely downwards (5), to go under the wrist and end at the top of the shoulder (6)—in this way completing the fixing of the dressing to the arm, and at the same time acting as a sling for the hand. A bandage six yards long generally does this exactly.

Pins are now inserted at all the necessary points, more especially where the bandage passes over the shoulder and under the elbow. The arm and dressing are then fixed securely to the side by a binder of calico, broader than the length of the

FIG. 44—DRESSINGS APPLIED AFTER EXCISION OF MAMMA AND AXILLARY GLANDS, TO SHOW THE ARRANGEMENT OF THE DRESSINGS AND BANDAGES.

The turns of bandage are numbered, and arrows are placed on them to show the direction in which they run.

upper arm, passing round the body, below the axilla of the other side, and pursed up and pinned above the shoulder, and below the elbow of the included arm. Thus perfect rest is procured, and no elastic bandage is required.

When the axillary incision is soundly cicatrised, and the discharge has become small in amount, the axillary dressing or the breast dressing No. 2 may be applied, the arm being simply supported in a sling.

An *axillary* dressing must be applied partly to the chest and partly to the upper arm, and made to fold over the top of the shoulder. It requires an elastic bandage (Fig. 46).

Elastic bandage is not as a rule required for *dressings on*

the extremities, because the limb operated on is generally placed on a splint for a few days, in order to procure absolute rest till healing by first intention is complete. Thus the movements which it is the function of the elastic band to neutralise are avoided, and the constriction of the elastic is also avoided. With regard to this constriction, however, the elastic need never be applied so tight as to produce œdema; indeed, I have more than once seen œdema which was present before an operation subside afterwards, even although an elastic bandage was used. Where the patient is allowed to move the extremity—as, for instance, when he is allowed to walk after a small operation on the lower extremity—an elastic bandage is

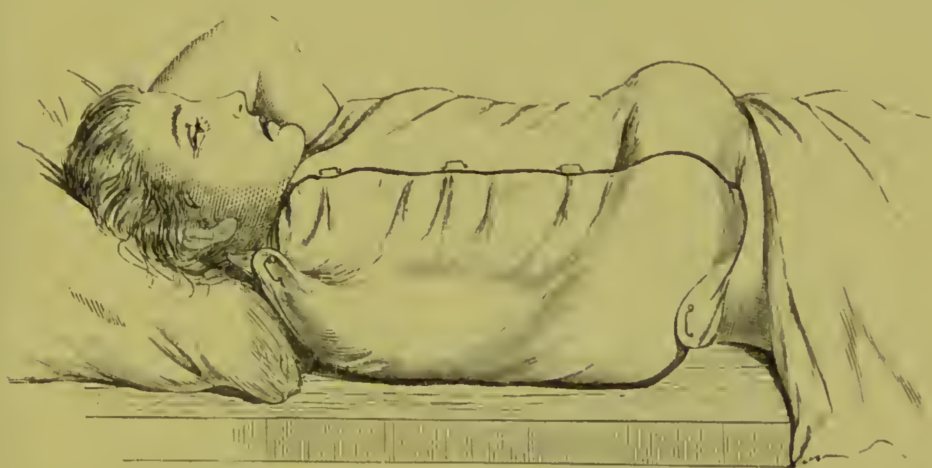

FIG. 45.—BINDER APPLIED OUTSIDE THE DRESSING REPRESENTED IN FIG. 44, SO AS TO KEEP THE PARTS AND DRESSING AT REST.

absolutely necessary. In the case of the lower extremity, the padding at the upper part of the splint should be covered with macintosh, and the foot of the bed should be supported on blocks. In this way all the discharge flows upwards, and as it cannot soak into the padding of the splint, it is shed on the draw-sheet soon after it has reached the edge of the dressing, and thus one can ascertain accurately whether or not it is necessary to change the dressings.

The dressing required for *psoas abscess* opened above Poupart's ligament is one of the most important dressings, as well as one of the simplest illustrations of the method of applying the elastic bandage. I may say here with regard to this method of opening psoas abscess above Poupart's ligament that there

are two reasons for choosing this situation. In the first place, the old rule that these abscesses must not be opened early is now done away with, and under truly aseptic treatment, as soon as fluctuation is detected, an operation is performed of a similar nature to that for tying the external iliac artery, and the abscess is opened after a careful dissection. The sooner the abscess is opened the better, for the abscess cavity is thus smaller than if the surgeon waits till the pus has burrowed its way into the thigh; and, further, so long as the pus is there it irritates by its tension, and thus keeps up the chronic in-

FIG. 46.—DRESSING IN CASES OF OPERATION ON THE AXILLA ALONE.

In this case an abscess has been opened and the position of the drainage tube is indicated by dotted lines. The edge of the dressing has also been dotted in.

flammation in the spine. This, then, is one reason why the opening leading into these abscesses is generally above Poupart's ligament. Another is, that even supposing the abscess to be pointing in the thigh, it ought to be opened as far as possible from sources of putrefaction, and the most convenient place in this respect, as well as the best for the attachment of a dressing, is the neighbourhood of the anterior superior spine. I shall hereafter discuss the reasons why it is thought best by some surgeons, more especially by Mr. Chiene, to try to get

at these abscesses from behind either by perforating the ala of the innominate bone or by getting at the pus above the crest of the ilium. Such a method has advantages both by providing a dependent opening, and also by leaving a shorter channel between the seat of the disease and the cutaneous surface.

The dressing applied when the opening is in the neighbourhood of the anterior superior spine extends from the middle line in front to the middle line behind. It reaches as high up as the lower border of the ribs and as low as about three inches below Poupart's ligament. Special masses of gauze are placed

FIG. 47.—DRESSING IN A CASE OF PSOAS ABSCESS OPENED ABOVE POUPART'S LIGAMENT, SEEN FROM THE FRONT.

The position of the drainage tube is indicated by dotted lines.

in the neighbourhood of the pubis, which is also shaved on that side. The dressing is fastened on by a spica bandage with circular turns around the thigh and abdomen. The elastic bandage is applied accurately to the edge. It begins, say, at the upper and anterior angle of the dressing, runs vertically downwards along the anterior edge; then, passing back round the inner side of the thigh, it encircles the thigh, thus fixing the lower border; then it runs vertically upwards behind till it reaches the upper posterior angle; then, being held there, it is carried round the abdomen. The two ends of the two vertical pieces are fastened to the circular piece by pins, and pins are also applied at all the angles and along the edge where neces-

sary. In some deformed persons shoulder straps are necessary to prevent the dressing from slipping down (Figs. 47 and 48).

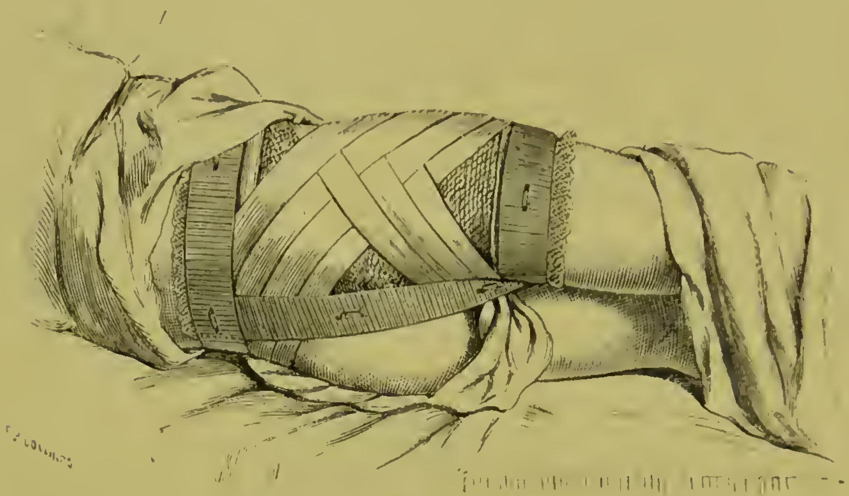

FIG. 48.—PSOAS ABSCESS DRESSING (FIG. 47), SEEN FROM BEHIND.

In *lumbar abscess* straps must pass over the shoulders to prevent the dressing from slipping down, and between the thighs to prevent it from slipping up (Fig. 49).

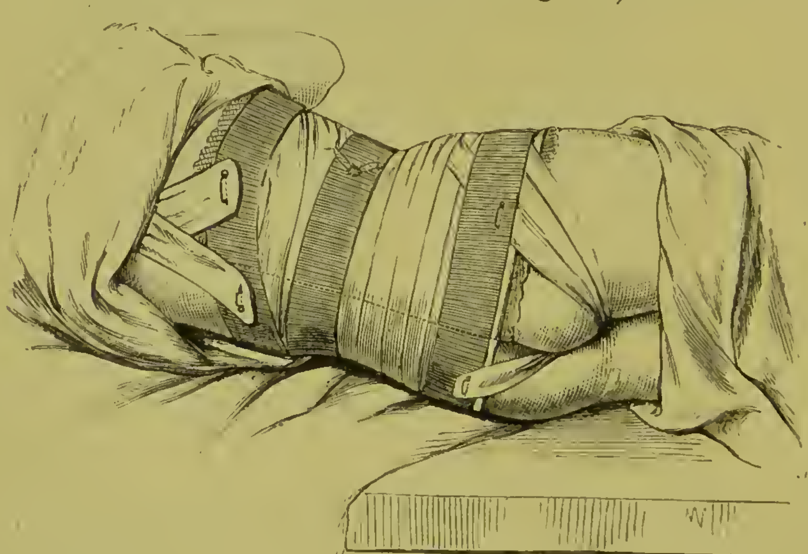

FIG. 49.—DRESSING IN A CASE OF LUMBAR ABSCESS, SEEN FROM BEHIND.

The position of the drainage tube is indicated by dotted lines; the vertical dotted lines at the middle of the back indicate the edge of the dressing.

In *abscess of the hip-joint* the arrangement of the dressings is much the same as in psoas abscesses, except that they pass lower down and not quite so high up. As a long splint is

generally in use, an elastic bandage is unnecessary, unless in children (Fig. 50).

Where abscesses are opened near the top of the thigh on the inner side, and are thus pretty near sources of putrefaction, large masses of gauze must be applied between the orifice and the perineum, and an elastic bandage carefully fastened along the upper edge.

In *operations for hernia, varicocele, and on the scrotum*, in the male, there is one form of dressing which is generally applicable. In the first place, no protective is used, on account of the immediate vicinity of sources of putrefaction, as has been previously explained. The gauze applied to the wound, instead

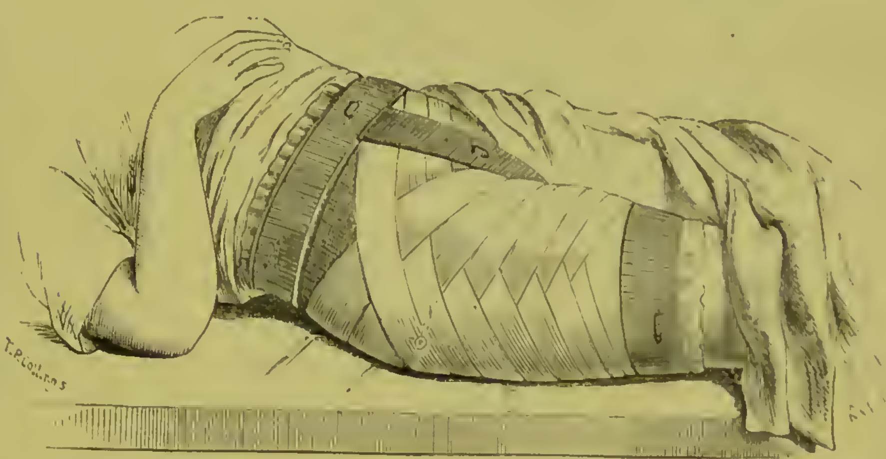

FIG. 50.—DRESSING IN A CASE OF HIP-JOINT ABSCESS, WITH ELASTIC APPLIED.

The dotted part shows the position of the wound.

of being merely wet with carbolic lotion, is steeped in 1–5 or in 1–10 solution of carbolic acid in glycerine, and this is wrapped around the penis and over the scrotum. This gauze sticks to the skin and does not become detached with the movements of the body, while it is more powerfully antiseptic than the ordinary carbolic gauze. Then a mass of gauze is rolled into a ball, and this is suspended in the centre of a long strip of gauze. The ball is placed in the perineum behind the scrotum, and the strip of gauze passes up in each groin. This strip retains the pad in position (Fig. 51). The pad serves the double purpose of supporting the scrotum and receiving the discharge, which passes chiefly downwards. The

hollows having been filled up with loose gauze, the general dressing is applied. A hole is cut in this dressing towards the

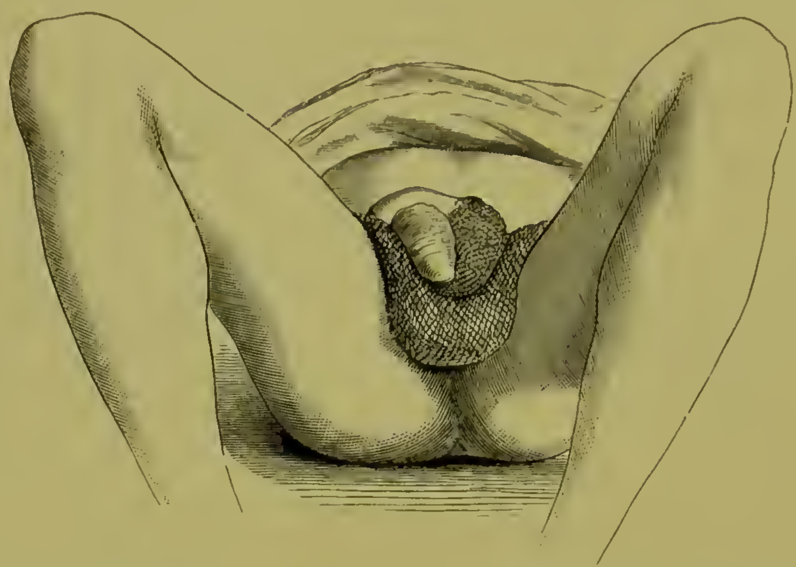

FIG. 51.—DEEPER PART OF THE HERNIA AND SCROTAL DRESSINGS.

Left side of scrotum covered with gauze soaked in carbolised glycerine. Mass of gauze in the perineum enclosed in a roll of gauze.

upper border, and the penis is passed through this hole, and thus helps to keep the dressing in position. The dressing

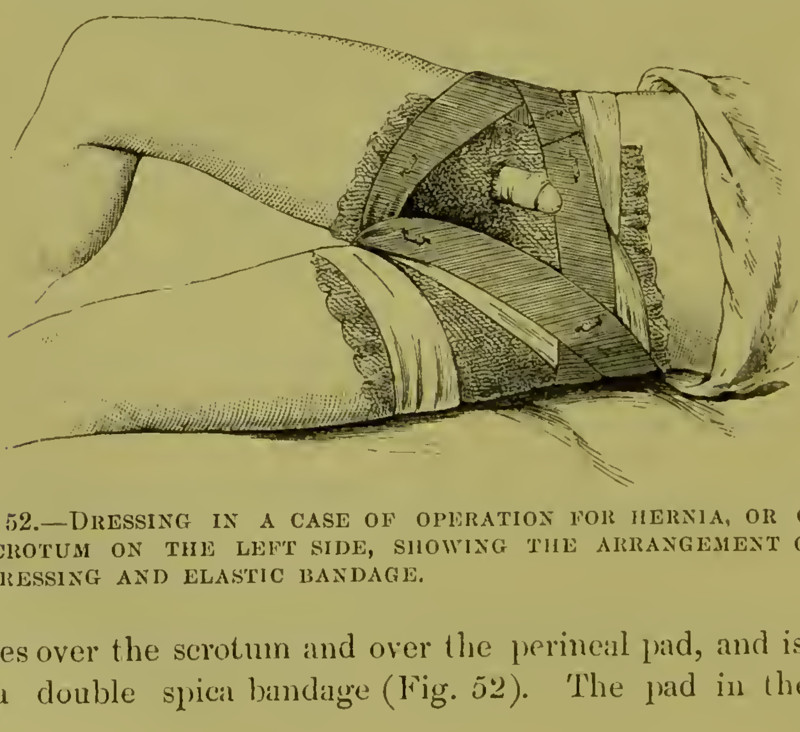

FIG. 52.—DRESSING IN A CASE OF OPERATION FOR HERNIA, OR ON THE SCROTUM ON THE LEFT SIDE, SHOWING THE ARRANGEMENT OF THE DRESSING AND ELASTIC BANDAGE.

passes over the scrotum and over the perineal pad, and is fixed by a double spica bandage (Fig. 52). The pad in the peri-

neum is fixed there by a St. Andrew's cross. The elastic bandage is applied in the form of a St. Andrew's cross in the perineum, and of a double spica (Fig 53). The bandages, dressing, and perineal pad are carefully pinned together in the perineum.

The methods of managing *excisions of joints*, operations for ununited fractures, &c., in the lower extremities, are very important. Here perfect rest must be combined as far as possible with the aseptic treatment. For two or three days after an

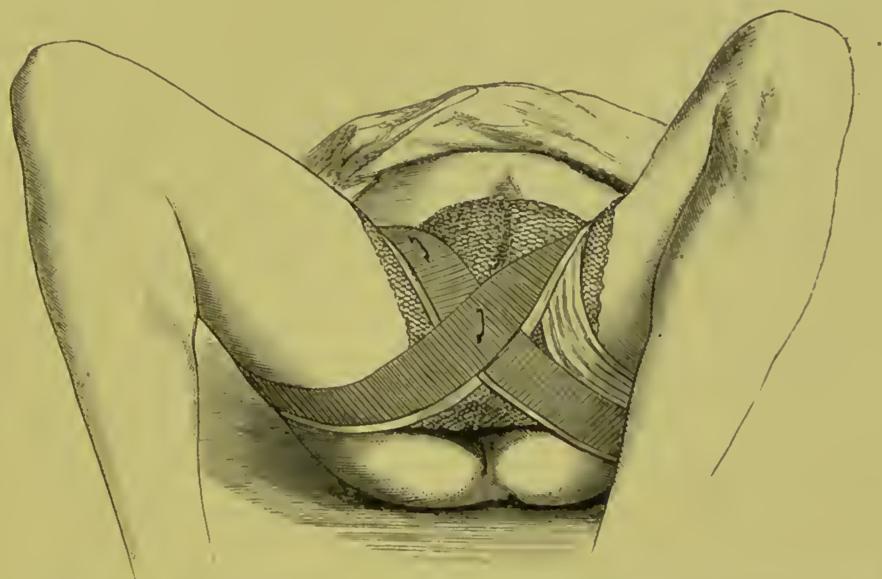

FIG. 53.—DRESSING IN HERNIA CASES OR IN OPERATIONS ON THE SCROTUM, SHOWING THE ARRANGEMENT OF THE BANDAGES IN THE PERINEUM. (SEEN FROM BELOW).

operation it is better to change the dressing, which is the ordinary gauze dressing applied round the limb, simply by lifting the limb, because there is generally a large amount of bloody and serous oozing at first. After a few days this oozing has become much diminished in amount, and the dressing is then accomplished in the following manner:—A Gooch's splint is padded above and below the situation of the wound, the part opposite the wound being left unpadded. The whole splint and padding is covered with a piece of macintosh cloth, and is firmly fixed to the posterior aspect of the limb above and below the situation of the wound. Behind the wound, at the part where the padding is deficient, masses

of gauze of sufficient thickness are arranged transversely and superficial to the macintosh. These pieces are three or four or more in number, and they act as padding for the splint, and at the same time as an antiseptic dressing (Fig. 54). When the dressing is changed, a piece of gauze is pinned to each

FIG. 54.—SPLINT FOR EXCISION OF KNEE, READY FOR APPLICATION.

The splint is padded at the upper and lower parts, and the splint and padding are covered with a piece of macintosh cloth. The space opposite the knee is filled with masses of gauze arranged transversely and superficial to the macintosh.

of the old pieces, and then the old piece being pulled out the new is pulled in, and thus the limb is never left without support (Fig. 55). Over the front of the limb an ordinary gauze dressing of suitable size is applied.

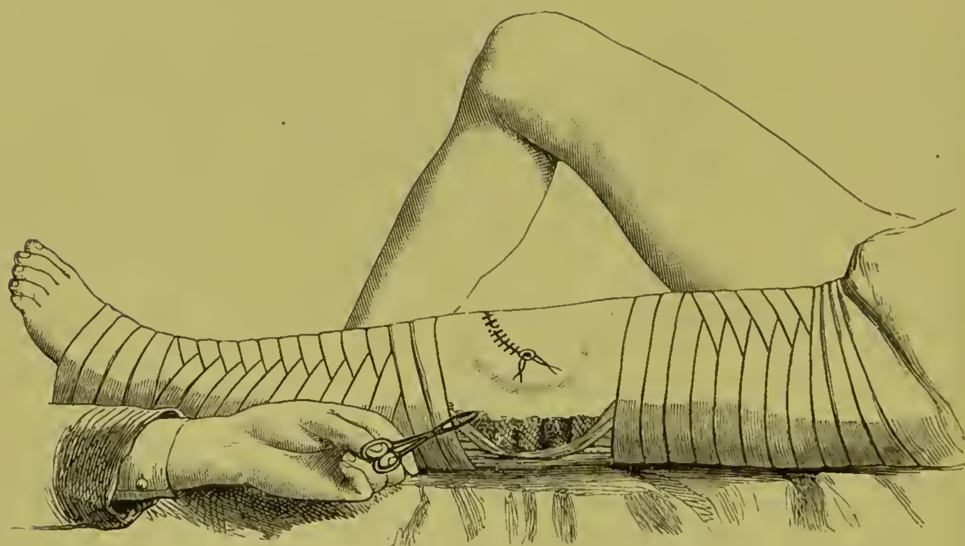

FIG. 55.—SPLINT APPLIED IN A CASE OF EXCISION OF THE KNEE.

This shows the method of changing the dressing. In the first way described a mass of gauze would be pinned on to the end of the old piece on the other side of the limb, so that as the old piece is pulled out the new is pulled in, or it may be arranged in the second manner described, and shown in Fig. 54—viz., a piece only extends to the middle line behind, and as soon as each is pulled out a fresh piece is pushed in.

Another more convenient way in which this may be managed with even less movement is to have each of the masses of gauze mentioned in the former paragraph divided in the middle line, and thus the half of each mass is pulled out at a time and a new piece substituted (Fig. 54).

Another way is to apply a wire splint next the skin, fix it there, and then apply the dressings outside.

When the discharge becomes still less the limb may be put up in plaster of Paris, a window being left for dressing.

Excision of joints is now, however, rarely performed, for with aseptic treatment an incision into a joint and the insertion of a drainage tube is generally sufficient in cases where formerly excision or even amputation would have been required. Several advantages are thus gained, among the most prominent of these being absence of shortening of the limb (and this is most important in children), and often a certain and even a considerable amount of motion in the joint afterwards.

It may be mentioned here that Mr. Knowsley Thornton in *ovariotomy* cases does not apply a bandage round the abdomen. He fastens the dressing with adhesive plaster, and does not change it for a week, by which time healing is generally complete, except where the stitches are.

Such are the chief points as to the application of antiseptic dressings in different situations. I must now say a few words as to the aseptic treatment of abscesses.

I have already referred to the question of the necessity for a dependent opening, and I pointed out that, as the discharge from an abscess treated aseptically is not irritating, because not putrid, it does little harm even though left to well out, instead of being permitted to flow out through a dependent opening. In fact, aseptic surgery has altered the relative importance of the questions to be considered in selecting a situation for opening an abscess; and now the chief point to be looked at is not whether the orifice of the tube is in the most dependent position possible, but whether it is at the point furthest removed from sources of putrefaction—*i.e.* whether there is the greatest possible space for the overlapping of the antiseptic dressings.

Indeed, in some abscesses pointing near such canals as the pharynx, anus, &c., it is better to make an opening in healthy structures at some distance from the abscess, and burrow a channel into it, than to make an incision directly into the abscess cavity.

I saw a striking example of this in Edinburgh several years

ago. A boy was admitted into the infirmary with retropharyngeal abscess connected with occipito-atloidean disease. The abscess was on the point of bursting into the pharynx. Mr. John Chiene, who had charge of the case, instead of opening the abscess at the only place where it was pointing, viz., in the pharynx, cut down behind the sterno-mastoid, and burrowed into the abscess cavity from behind. The abscess followed a typical aseptic course, and the patient recovered completely. Thus then the great rule in selecting a situation for opening abscesses is to make the incision as far as possible from sources of putrefaction.

When opened, instead of dealing tenderly with the pyogenic membrane as was formerly done under the impression that it was a hurtful thing to injure it, we now empty the cavity thoroughly, especially in the case of chronic abscesses, in order to get out all curdy masses of pus, &c., which may have gravitated to the bottom of the abscess. When this is done opportunity is given for the rapid adhesion of the greater part of the wall of the abscess cavity, and thus in a very short time there is merely a sinus left leading down to the seat of disease.

There is no necessity for washing out the cavity of an abscess, as is done in so many quarters. To do so is simply to irritate the pyogenic membrane unnecessarily without securing any corresponding benefit. Indeed, it might give rise to such an amount of oozing from the wall of the abscess as would wash out all the carbolic acid from the dressings in a very short time, and thus lead to the putrefaction of the discharge. The treatment by hyperdistension, while erroneous in theory, is very dangerous in practice, as the fluid may be forced into the cellular tissue, and lead to diffuse inflammation and even gangrene, or to carbolic acid poisoning and death.

The greatest care must be taken in the drainage of abscesses. In the case of a large psoas abscess the surgeon should introduce the largest sized drainage tube in the first instance. This tube may be changed for a smaller in a few days. It ought not to be removed for the first time till at least three days have elapsed since the abscess was opened, otherwise there may be great difficulty in replacing it. It should not be shortened till it is found to be absolutely impossible to get it in fully. When-

ever this is the case a piece must be cut off from the end. (Here I speak of chronic abscesses, an acute abscess heals in a week or ten days.) In some cases, where the same tube is left in for a week (where the case is only dressed once a week), some difficulty will be found in withdrawing it, owing to the granulations having grown in at the holes and holding it in position. In this instance the guide as to shortening is lost, because the tube cannot be pushed out; and therefore it will be found best in old cases to use a tube having holes only close to its inner end. This cannot be held, and is gradually pushed out as the sinus heals from the bottom. If on removal of a tube the discharge is found to increase in quantity, the tube must be reintroduced.

As the incision into the abscess is merely large enough to admit the tube, there would be no reason for using protective; and therefore the wet gauze is applied directly over the orifice of the tube. A tube is the only form of drain suitable in these cases.

The precautions required in order to ensure an aseptic result are precisely the same as in the case of wounds.

In changing the dressings the same rules are followed as were formerly described with regard to incised wounds. Chronic abscesses, more especially abscesses connected with diseased bones, are extremely tedious; but nevertheless, as a rule they ultimately recover. The same care must, however, be taken from first to last. It is never safe to change the carbolic dressing for a boracic one, however superficial the wound appears to be. In the case of spinal abscesses absolute rest in the recumbent posture must be maintained till healing is complete; and as the cases generally extend over many months it is well to warn patient and friends before commencing to treat the case. Whether the rule as to the maintenance of the recumbent posture may not be modified by the use of Sayre's jacket, or even without it, is now a question. Lately in two cases which had been under treatment for a long time, and in which all uneasiness in the spine had passed off, Mr. Lister allowed the patients to get up before healing was complete, and without any bad results.

Empyema does particularly well under this dressing. I

mention it, in order to state that a metallic drainage tube with a shield like a tracheotomy tube, and with lateral holes, is the best because the india-rubber tube may get compressed between the ribs or be too abruptly bent where it passes into the interior of the pleural cavity.

There are some cases in which neither the gauze dressing nor the boracic can be employed, but which may nevertheless be treated aseptically. I refer especially to abscess in the perineum or by the side of the anus.

Abscess in the perineum may be treated aseptically with very satisfactory results. The abscess is opened under the spray, and a piece of lint dipped in 1–5 carbolic oil or 1-10 carbolic glycerine is introduced into the cavity to act as a drain. Outside this two or three layers of lint soaked in 1–5 carbolic oil or 1–10 carbolic glycerine are applied, and fixed with a T bandage. Should this become displaced or wet with urine, &c., the patient pours a little carbolic oil or glycerine over the wound and over the lint, and replaces the dressing. No spray is required in changing the dressings. On the third day a piece of lint dipped in carbolic oil is laid over the wound, and a pair of oiled forceps is slipped under the lint to seize and withdraw the plug; or the plug may simply be pulled out under the spray. Carbolic oil or glycerine 1–10 is then used for dressing, and when the wound has become superficial boracic or salicylic ointment is employed.

The same method of dressing is employed in abscesses beside the anus. In this case, when the patient defæcates, he holds aside the dressing, defæcates past it, wipes the parts with 1–20 carbolic lotion and then with 1–10 carbolic oil. He then soaks the dressing with the oil, or applies a new dressing. (The glycerine and carbolic acid may also be used.) The result of this method of treating these abscesses is often excellent, fistula in ano being apparently often avoided when the abscess is taken in time.

So much for wounds made by the surgeon and their treatment. I now come to the consideration of *wounds produced accidentally*. Here the problem is different from and much more difficult than the former. In the cases we have just

been considering we had merely to keep out the septic particles; in the present instance these particles have already gained admission, and therefore we have not only to prevent the entrance of more but also to destroy those already present.

This is done by washing out the wound with 1–20 carbolic lotion, provided it be recent, *i.e.*, made within twenty-four hours, and then treating it like a wound made by a surgeon.

This washing out of the wound must be done very thoroughly. It is best carried out by using a syringe with a catheter attached to it. The point of the catheter is introduced into all the recesses of the wound and the 1–20 lotion is injected through it, and thus comes thoroughly in contact with all parts. There must be no attempt to distend the cavity, as, for instance, by shutting the orifice of the wound around the syringe, for the fluid might be forced into the cellular tissue and lead to inflammation or even sloughing. The opening must be left perfectly free and enlarged if necessary. Should there be any shreds of tissue, they had better be cut off, and if there be much dirt ground into the tissue, it must be got rid of by means of a nail brush. The injection and the subsequent procedures are carried out under the spray.

If the wound was made twenty-four to forty-eight hours before being seen, a stronger solution is employed, viz., the 1–5 spirituous solution. This is used in the same way as the other.

Having thus got the wound pure the question of stitching it up arises. The answer to this question varies according to the parts injured. As a rule, in injury of the soft parts, a drain is introduced, and the same accurate stitching employed under the spray as was described on a former page. More especially is this the rule in scalp wounds, where most brilliant results may be obtained by the use of catgut drains and accurate stitching. The rest of the treatment is the same as in operation wounds.

Where the wound is much contused, the same rules apply as to purification, but it must not be stitched up. After purification a drainage tube is inserted if necessary, the wound is left open, a piece of protective is placed over it, and the dressing applied in the usual manner.

I have mentioned the methods to be employed when the wound is seen within the first forty-eight hours. It may be, however, that it does not come under notice till putrefaction already exists. In this case it may be purified by the introduction of iodoform suspended in water by the aid of alcohol, or if superficial, by stuffing it thoroughly with lint dipped in 1–5 carbolic oil. This dressing repeated for several days generally converts it into an aseptic wound. In most cases it is best to apply iodoform or the chloride of zinc solution.

Certain special wounds call for attention.

Compound fractures are the wounds in which this treatment was first applied, and in which excellent results can be obtained. There are a few special points to be noted. In purifying the wounds great pains must be taken. Any dirt must be carefully scraped or scrubbed out. All blood clots ought to be turned out as completely as possible. The ends of the bones are cleaned, and if they cannot be returned or got to fit, portions should be sawn off. The ends may be tied together with silver wire. The parts ought to be well kneaded as the carbolic lotion is injected through the catheter, in order to diffuse the lotion as much as possible into all the recesses of the wound. No stitches are inserted, but on the contrary, free drainage by tubes is used. The same sort of dressings and apparatus are employed as in excisions.

Wounds involving tendons, nerves, or muscles, are treated in the same manner as others, and the ends of the divided muscles, tendons, or nerves, ought to be stitched together with catgut, and the position of the part so arranged as to avoid dragging on these stitches.

Wounds of joints are very important. When recent no operation (excision or amputation) is required in the first instance. As a rule the joint may be saved, and perfect movement obtained by washing it out very thoroughly with carbolic lotion 1–20. The wound in the joint is enlarged if necessary. Where several hours have elapsed since the accident (more than eight or ten hours), it is well to employ the spirituous solution. A drainage tube is introduced into the joint, but no stitches are used. After a few days, when the discharge has diminished,

the drain is removed. In about three weeks, or earlier, passive motion ought to be begun, otherwise the adhesions outside the joint may become so strong as to require to be broken down under chloroform.

Compound fractures of the skull are treated in the same manner as compound fractures elsewhere, purification being attempted with 1–20 carbolic lotion. The dura mater may be freely dealt with without fear of inflammation, for the irritation of carbolic acid is only very transient. Bleeding vessels are secured by catgut. Should one of the great sinuses be wounded, a graduated compress of catgut arrests the hæmorrhage satisfactorily. This I have known to act very well in a case of wound of the longitudinal sinus, occurring during the operation of trephining over the seat of an old injury.

Wounds penetrating the thoracic cavity are much more difficult to treat. Should the wound penetrate the lung, and should the lung protrude, the exposed parts and those around are purified with carbolic lotion 1–20. In deciding as to the returning of the injured lung and the stitching up of the wound, the surgeon must be guided by the circumstances of the particular injury. In some cases, if the wound in the lung were superficial, the edges of the divided visceral pleura might be stitched together with fine catgut, the lung returned, and the external wound closed. Where a large bronchus is injured it might be better practice to leave the lung in the wound, and leave the wound open.

Where there is merely a wound of the parietal pleura, and where the lung is not wounded, the external wound only is purified and is closely stitched, in the hope that union by first intention may occur, that the air may be absorbed, and that any septic dust present in the pleural cavity may be unable to cause mischief.

Wounds of the abdomen are variously treated, according as there is or is not protrusion of the contents. Where there is no protrusion, and where there is no reason to suspect injury of the viscera, the external wound ought to be purified and closely stitched, so as to get primary union throughout, no drain being used.

Where the intestines protrude, they ought to be carefully

bathed in warm carbolic lotion 1–30 or even 1–20, and if there be no injury of them in any part they may be returned. If they are cut, the gut may be stitched with catgut by the glover's suture.

If the omentum protrudes, opinions vary as to the treatment. When it can be returned do so after thorough purification, and then stitch the abdominal walls, including the peritoneum, close together. Where, from adhesion or other sufficient cause, this cannot be done, or where the omentum is very dirty, I should, from a research into the consequences of unreturned omentum by Dr. Kenneth McLeod, of Calcutta, consider it the safest practice, especially in the case of a person with strong muscular parietes, to stitch the deepest parts of the omentum to the deep part of the wound, cut off the remainder and close the skin over all.

If internal hæmorrhage is going on, apparently from the mesenteric vessels, the wound may be enlarged and the bleeding point sought for. Simon advised that in bleeding from the kidney, the injured organ should be excised; this suggestion was never put into practice, but nevertheless it is one well worth bearing in mind.

Such are the chief points to be attended to in recent wounds; there remains for consideration the class of cases in which putrefaction has been present for a long time. I refer to cases of putrid sinuses, generally connected with diseased bones or joints. An attempt may be made to purify these during the course of an operation, and sometimes when the sinuses are few and uncomplicated, and where all the dead bone is removed, this attempt may be successful. The sinus is scraped out with one of Volkmann's sharp spoons (Fig. 56), and all the granulation tissue, as far as possible, removed. The raw surface of the sinus, &c., is then washed out with the chloride of zinc solution, which is applied thoroughly to all parts, and a gauze dressing is used, in the hope that putrefaction has been thus eradicated. The spray should be employed during the whole procedure.

If this is successful, well and good. If not, boracic ointment (at first full strength, afterwards half) or salicylic oint-

ment, covered with boracic lint, is the best dressing, indeed, it is the best dressing in all cases where strict aseptic measures are inapplicable.

The aseptic treatment of burns varies according to the degree and extent of the injury. In any case, unless where the burn is very extensive and where the parts are extremely dirty (necessitating scrubbing of the surface and consequent shock, and also risk of carbolic poisoning), an attempt should be made to purify the surface with 1–20 carbolic lotion. This having been done, if the surface is small, boracic ointment (full strength) and boracic lint form a convenient dressing. When the extent of the burn is greater, wet boracic dressing (wet boracic lint used as water dressing—covered by gutta-percha

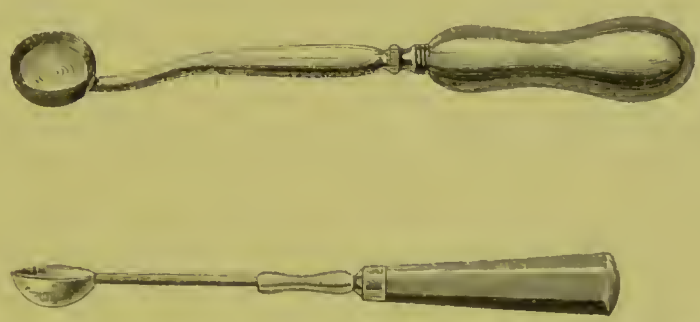

FIG. 56.—TWO FORMS OF SHARP SPOONS, A LARGE ROUND ONE AND A SMALL OVAL ONE.

tissue or macintosh) is the most suitable. The wet boracic dressing is also applied in those cases where, on account of the extent of the burnt surface and the amount of dirt, purification with carbolic acid is not advisable. Where the surface is thoroughly charred and where the wound is not very extensive, boracic ointment or carbolic oil 1–10 are the best dressings. The objection to carbolic oil, which was formerly used in all cases, is that, when the surface is large, there may be a fatal absorption of carbolic acid.

In the after-treatment the sores are dressed with boracic dressings (protective and boracic lint, or better, in the first instance, boracic ointment) just as in the case of ulcers.

The rules as to the treatment of gangrene are altered in

aseptic surgery, and this is more especially the case with senile gangrene. Should symptoms of senile gangrene set in, say in the lower extremity, the skin of the foot, toes and leg, are thoroughly cleansed with 1–20 carbolic lotion. This must be done very efficiently. All the folds about the nails, &c., must be carefully cleansed and washed. This having been done, the whole limb and foot are enveloped in a large mass of carbolised cotton wool (carbolised in a 1 per cent. ethereal solution of carbolic acid). This being pure in its substance, and being applied over a pure surface, completely shuts out causes of putrefaction. The carbolic acid soon flies off, and then the cotton wool acts simply as a filter, while it protects the part from unequal pressure and retains the heat. This may be kept on for any length of time, and so long as discharge does not extend to the surface or the gangrene above the limits of the dressing, the part remains sweet, and very often the gangrene, which in the first instance threatened to involve the whole leg, becomes limited, and there may even be merely a small cutaneous slough. In any case, as a rule, the gangrene does not go on spreading as it does when treated in the usual manner, and for this reason:—Suppose that the part is not treated aseptically, the tissue at the edge of the dried gangrenous mass becomes putrid, the living tissue in the neighbourhood is very weak, the putrid material acts on it like a caustic, destroys its vitality or excites an inflammation which kills it, and so the gangrene goes on spreading, till at length parts are met with of sufficient vitality to resist this action of the putrid materials. Then a line of demarcation is formed. On the other hand, when the gangrenous parts are not putrid, the weak parts in the vicinity, which would to a certainty have died in the former case, retain their vitality and gain strength. Thus also the rule of never amputating in senile gangrene, except to trim a stump formed naturally, is done away with, and it is generally better to amputate as soon as it is clear to what extent the tissue is dead, rather than to subject the patient to the continual pain and irritation arising from the presence of the dead piece. The same reasoning applies to cases of traumatic spreading gangrene. This is only one instance of how completely many current ideas as

to surgical pathology and treatment are reversed, when means are taken to render the dust of the atmosphere inert before it reaches a wound.

In treating nævi great benefit is obtained from the injection of pure carbolic acid. The nævus is first thoroughly cut off from the circulation by ligatures tightly applied around its base, and then half minims of pure carbolic acid are injected into various parts of the tumour. Ten minutes or so having been allowed to pass, in order to ensure complete and firm coagulation, the ligatures are divided and removed, and the punctures are touched with collodion. The surface being left completely dry, any slough which forms becomes absorbed or separates as a crust after some time, the part beneath being found to be a scar.

The same method answers excellently in the treatment of varicose veins. A tourniquet having been firmly applied around the upper part of the limb in order to arrest the circulation, the vein is punctured at various parts, and half minims of carbolic acid are introduced into it. The tourniquet is kept on for ten minutes after the injection is completed. Coagulation and a slight degree of inflammation are thus induced, but this, so far as I have seen, never goes to any dangerous extent, and is followed by at least temporary cure. I have not known any case return with reformation of varicose veins.

A dissection or post-mortem wound does not give rise to bad results if the wound be instantly purified with 1–20 carbolic lotion.

CHAPTER VI.

ASEPTIC SURGERY—MODIFICATIONS.

Country practice: *How to dispense with the spray during the operation—and during the after-treatment: How to render the dressings less frequent.* Is the aseptic method applicable in war? *Mr. Lister's suggestions: Esmarch's plan: Reyher's method.* Development of Aseptic Surgery in Mr. Lister's hands: *Compound fractures—Pure carbolic acid—Formation of crust—Carbolic putty—Lead plaster—Lac plaster, syringing wounds with carbolic lotion, protective, catgut ligatures, method in* 1870—*Present method in the main introduced in* 1871—*Further introduction of wet gauze, steam spray, elastic bandages: abscesses—Method of opening them under carbolic oil: wounds.*

Such are the methods usually employed in carrying out the Listerian principle in hospital or in private practice. It is, however, said to be difficult of application to country practice, and we must therefore enquire in what way it can be made easier. The difficulties urged are that the spray is too heavy to carry: that it is not always easy to return a long distance to see a patient on the day after the operation, and that the dressings are too expensive for the lower classes. We must therefore, in some way or other, render the dressings very infrequent, so as to avoid expense and unnecessary visits, and we must try to dispense with the spray.

In the first instance in going to perform an operation or to treat a wound, the surgeon takes instruments with him, and he may, without any additional trouble, easily add a spray to the contents of his bag, and this spray may be left at the patient's house, and brought home again after the first dressing. But, suppose the surgeon has not a spray at hand. What is to be done? Well, he must use all the other precautions before described, and wash out the wound frequently with 1–40 carbolic acid lotion during the operation, and while the stitches

are being inserted; and then, before the piece of wet gauze is applied, he may distend the wound with the same lotion, the wet gauze being applied while this is still flowing out. At the same time I cannot see that, in the great majority of cases, it can be any great hardship to carry a spray to an operation.

During the after-treatment a spray is not necessary. The spray may be rendered unnecessary during the after-treatment in two ways. In the case where catgut stitches and catgut drains are used a deep dressing may be applied at the time of the operation, and may never require to be changed. This deep dressing is fixed down in some way or other, and is treated as a wound, the gauze being soaked with carbolic lotion every time the superficial dressing is removed, and then a piece of wet gauze larger than the deep dressing, and the general dressing are applied. Should it be necessary to remove the deep dressing, there is no necessity for the spray, if catgut drains be used, because there is no cavity into which air may pass. The deep dressing having been removed, carbolic lotion is allowed to flow over the wound till a guard is applied. Where a tube is used it is more difficult to do without the spray, for in that case there is an open orifice into which dust may fall, and be sucked into the interior of the wound, and further, when the tube is removed, air must enter to take its place. This may be avoided by the use of a syringe which constantly keeps a stream of carbolic acid lotion passing over the wound and over the drainage tube, till a fresh dressing is applied. Should it be necessary to remove the tube it is well, in addition to this constant flow of lotion, to cover the orifice of the tube with a rag dipped in the antiseptic lotion. The best way is to take a guard soaked in carbolic lotion and folded in several layers, and place this over the orifice of the drainage tube, extending on each side of it for a considerable distance. The tube is now seized with a pair of forceps through this rag, and as it is pulled out the rag is carefully tucked in around it, so as to compel the air, as it passes in to take the place of the drainage tube, to traverse the moist guard. This seems to me better than the method of slipping in forceps under the guard and pulling out the tube, the guard being well pressed down on it. In taking out wire or silk stitches, the

guard is pulled aside so as to expose the stitch, a little carbolic lotion is then dropped over the suture, and as the latter is withdrawn, a few drops of the lotion are applied to the orifice of the puncture.

These methods—the use of catgut stitches and catgut drain, and the employment of a permanent deep dressing, together with the hints in cases where a drainage tube or non-absorbable stitches are employed, suffice to render the operator independent of a spray.

Can we now render the dressings less frequent? This may of course be done to a certain extent by applying a larger amount of gauze, but the best way is to use sponges in the interior of the dressing for the purpose of absorbing and retaining the fluid. The deep dressing having been applied and fixed, a large sponge or several small ones are placed outside it, these sponges having just been wrung out of carbolic lotion; outside the sponges and extending well beyond them is a piece of wet gauze, and then the masses of loose gauze and general gauze dressing. In this way the discharge is retained in the interior of the dressing, and of course so long as it is there, and so long as the discharge has not reached the edge of the dressing, it is as safe from putrefaction as if it were in a pure flask. By the use of these sponges several days may be allowed to elapse, in many cases, before the first dressing is changed, though it is well in every case to change the first dressing on the day after the operation. When the dressing is changed these sponges are squeezed thoroughly, washed in carbolic lotion 1–40, and reapplied. By the use of sponges two or three dressings suffice for the treatment of most operation wounds.

By the use of salicylic jute in large masses, the same avoidance of frequent dressings may be obtained, but this material is not very trustworthy as an antiseptic.

Thoroughly purified cotton wool, which may be obtained cheaply by impregnating it with sulphurous acid fumes, applied in large mass may prevent the necessity of frequent dressing.

By the means described, the difficulties in the way of the adoption of this system in country practice may be overcome,

and instead of causing additional expense to a poor patient, it saves expense in many ways. The dressings required are so few that the price of the materials employed is not greater than that which would be necessary even if water dressing were used; and expense is saved in many other ways, as I shall mention in the latter part of this work, notably in the rapid healing, which is of course of the greatest consequence to the bread winner.

Is the Aseptic method applicable in War?

In the 'British Medical Journal' for September 3, 1870, Mr. Lister describes a method for the use of army surgeons. He suggests that the wound should, as soon as possible after the injury, be thoroughly washed out with 1–20 carbolic lotion, the surrounding skin being at the same time purified. Bleeding vessels are secured by catgut, by torsion, or by carbolised silk. While the wound is full of lotion, extract the bullet, clothes, &c. Then cover the wound with two or three layers of oiled silk, smeared on both sides with carbolic oil 1–5. Over this apply layers of lint soaked in the 1–5 oil, overlapping the oiled silk for about three inches in every direction, and about a quarter of an inch in thickness. This is covered with gutta-percha tissue, and the whole is fastened on with a bandage soaked in carbolic oil. This is the permanent dressing. Outside this, another and larger dressing of oiled lint covered by gutta-percha tissue is applied daily. During the first day apply fresh oil to the outer cloth once in six or twelve hours. On the following day the outer dressing is changed, carbolic lotion being introduced under the edge, as it is lifted, by means of a syringe; or carbolic oil may be poured in. After the first dressing use the 1–10 oily solution, and later the 1–20. On the second day oil is only applied once in twelve hours, after that it is applied daily for five or six days, and then once in two days.

In compound fractures use a wire splint next the deep dressing, and apply the fresh superficial dressings outside the wire. This splint need not be removed till union is complete, the oil being merely poured between the meshes when the dressing is changed.

FIG. 57 (FROM MACCORMAC).

Esmarch's first dressing for the wounded in battle. 1. Packet folded up. 2. Triangular bandage. 3. Gauze bandage. 4. Antiseptic tampon. 5. Tampon and square of oiled paper.

Esmarch, in Langenbeck's 'Archiv,' vol. xx. p. 171, proposes another plan of treatment.

He points out that the new form of bullets passing quickly through the clothes may not carry into the wound any causes of putrefaction. Therefore, if the wound is not examined by dirty fingers or instruments, and if it be seen at once, it may in most cases be regarded as aseptic. Starting on this principle, he suggests that each soldier should be provided with tampons of salicylic cotton, wrapped in salicylic gauze. Fig. 57 represents the contents of the packet of dressings which Esmarch proposes to supply to each soldier. At the front, when there seems any possibility of saving the limb, these tampons are introduced into the openings, and bandaged on without any preliminary probing or examination of the wound. Any other necessary apparatus is applied, and the patient sent to the rear. At the rear the skin around the orifice is purified with some antiseptic lotion, and if there is any necessity to explore the wound, as for removing bullets, splinters, &c., the tampon is removed under the spray, the wound washed out, and an antiseptic dressing applied. If there is no necessity for exploring the wound, the skin is merely purified, and then a mass of salicylic jute or other antiseptic material is applied without disturbing the tampon. If putrefaction occurs later the wound must be enlarged, and an attempt made to purify it.

Conservative surgery being more applicable with the aseptic method, the necessity for primary amputation at the front is less frequent, and as a rule exists only in the case of smashes from large balls. Esmarch considers that for such cases a sufficient supply of antiseptic materials should be present in the ambulance. Referring to those cases not treated aseptically which do well, and to the evils of investigating the wound at the first, Esmarch says: 'So weit ich habe in Erfahrung bringen können sind diejeningen Fälle, welche aseptisch verliefen, auf dem Schlachtfelde nicht mit dem Finger gründlich untersucht, sondern gleich verbunden worden, während solche Fälle bei denen wiederholte Untersuchungen vorgenommen waren, mir oft einen besonders schlimmen Verlauf zu nehmen schienen.'

Esmarch's method has been put to the test by Dr. Reyher

during the late Russo-Turkish campaign. His results were excellent, and will be referred to later on. He carried out the aseptic method in two ways, according to the nature of the injury and the treatment before the case came into the surgeon's hands. These are, either that the surgeon closes the wound without further treatment, merely disinfecting the surrounding parts, or else that he cleans out and purifies the track of the bullet, and afterwards makes provision for free aseptic drainage. In the first instance healing occurs under a crust; in the second, under a moist and antiseptic dressing.

The cases which are suitable for the first method of treatment—treatment by a crust—are those in which the wound is small, where no clothing has been carried in with the bullet, where the edges of the wound fall together as where the wound is more or less valvular, and where no examination of the wound by finger, probe, &c., has been made. In such a case the surrounding skin is carefully purified, and an attempt is made to obtain a dry crust either by allowing the blood to dry, or by aiding the drying by applying charpie, gauze, &c.; or the wound may be covered by a mass of salicylic wool or carbolic gauze. Reyher lays particular stress on the avoidance of probing or draining such wounds. On the contrary, any communication with the outer world should be shut off as soon as possible.

In many cases this 'occlusion' of the wound cannot be depended on, and the bullet track must be washed out, and treated in the way described under compound fracture, free drainage being carefully provided. This is chiefly the case where the missile has been travelling slowly, and where, consequently, the wound in the skin is not so small nor valvular, and where there is more likelihood of articles of dress being carried in with it; where, also, as Reyher puts it, the wound is open and 'the air has not only entered but must enter again.' This treatment is also necessary in cases where wounds have been examined with unclean fingers or instruments before reaching the ambulance.

It is thus evident that the spray is not required for the majority of cases, and indeed by following the lines previously indicated it may be entirely dispensed with. The gauze re-

quired for the dressings can be made in the vicinity, and for this purpose Reyher carried with him the machine for making gauze described in Chapter III., and had thus a constant supply of the freshly-prepared material. There is not much difficulty, therefore, with regard to the materials; the real question is how to have the cases treated aseptically from the very first. Reyher was able to overcome these difficulties by proceeding in the following manner. In the first place, instructions were given that wounds were never to be examined at the front, either with fingers or instruments, nor was any attempt to be made to extract a bullet. The only exceptions to these rules were cases where blood-vessels were injured, though even in these it was generally possible to apply an Esmarch's elastic band to control the hæmorrhage temporarily; and cases where the projectile had passed into the large cavities of the body, and, without wounding the contents, had remained in the wall of the cavity. In such a case the bullet ought to be extracted at once, lest it should fall into the cavity during the transit of the patient. 'For surgeons at the front there is only one line of treatment—to occlude the wound provisionally—to lay the wounded part in a suitable position on the litter, and to render it provisionally immovable. As provisional dressing the salicylic balls recommended by Esmarch are the best.' This method is chiefly suitable for cases where the soft parts alone are injured. Most of the serious cases can be attended to as a rule at the foremost ambulance.

The more surgeons become imbued with the true principles of aseptic surgery, and the more thoroughly they grasp antiseptic surgery in all its developments, so much the greater is the likelihood of obtaining aseptic results. Reyher's results show strikingly what can be done with the methods at present at our disposal. There can be no doubt that with improved methods and increased knowledge and experience, aseptic surgery will soon be universally carried out in war.[1]

It will be interesting now to trace the gradual development

[1] For a *résumé* of the opinions of army surgeons on the best methods of carrying out aseptic surgery in war, see a little pamphlet by Surgeon-Major H. Melladew, *Notes on Antiseptic Surgery in War*. London: Ranken & Co. 1881.

of this system in Mr. Lister's hands. This will not only be interesting but instructive, as showing how aseptic surgery may be carried out in circumstances where spray, gauze, &c., cannot be obtained.

The first cases in which the treatment was tried were compound fractures, and the first attempt recorded by Mr. Lister was made in March 1865. This attempt was unsuccessful, as Mr. Lister subsequently believed, from mismanagement.[1] The next case was a compound fracture of the tibia, caused by the wheel of a waggon passing over the leg. The external wound in this case was $1\frac{1}{2}$ inch long, and $\frac{3}{4}$ inch broad. A piece of lint dipped in carbolic acid (which was at that time only got as an impure liquid known as German creosote) was placed upon the wound. Four days later this lint was removed, and the wound dressed with lint soaked in water in which this impure carbolic acid was diffused. Five days afterwards a solution of one part of carbolic acid in from 10 to 20 parts of olive oil was used for four days, and then ordinary water dressing was resorted to.

The aim in this case was to form a crust consisting of lint, carbolic acid, and blood, which would protect the deeper parts from putrefaction. In this instance, however, the scab came off on the fourth day, and in order to avoid this occurrence, Mr. Lister, in his next cases, applied a piece of lint dipped in pure carbolic acid as before, large enough to overlap the sound skin for a quarter of an inch all round, and covered with oiled paper, applying for the first four days outside this arrangement a piece of lint soaked in pure carbolic acid. The crust was left untouched till the eleventh day, and then water dressing was used.

It now became evident that, owing to the volatility of the carbolic acid, means were required to prevent its evaporation. Accordingly, in the next cases a layer of sheet lead or of block tin was applied outside and overlapping the crust.

Up to this time there was no attempt made to purify the interior of the wound in the first instance. How was it, then, that no putrefaction occurred? Probably for the following reasons. It may have been that no septic particles had got

[1] See numbers of the *Lancet* for 1867.

into the interior of the wound because the blood flowing constantly out would prevent the entrance of solid particles, which would thus only be present if dirt was introduced at the time of the accident, or if much movement of the fragments occurred afterwards, with consequent introduction of air. Further, if any septic particles were present they may not have been able to produce any effect in the presence of healthy living blood clot. (This will be discussed hereafter.) Lastly, the strong carbolic acid, though applied only to the surface, rapidly spreads into the interior even to a depth of one or two inches.

The necessity for purifying the deeper parts of the wound soon became evident. A case was admitted in which, when the tissues around the wound were pressed, bubbles of air escaped along with the blood. Here Mr. Lister squeezed out as much of the clotted blood and air as he could, and then applied a piece of lint dipped in pure carbolic acid, slightly larger than the wound, and over this the piece of block tin. The crust was daily painted with carbolic acid, the tin cap being reapplied on each occasion. This treatment was continued for three weeks.

This purification of the deeper parts was carried out more thoroughly in the next case, in which it was necessary to saw off a portion of the ulna, and here the whole of the interior of the wound was swabbed out with pure carbolic acid. In this case Mr. Lister first became acquainted with 'antiseptic suppuration,' *i.e.* suppuration due to the irritation of the antiseptic applied. (The evil effects of the pure caustic carbolic acid in causing excoriation of the skin had been already noticed.)

Finding that no harm resulted from the free application of pure carbolic acid to the interior of the wound in the last case, the next which came under notice was more thoroughly treated, the contused parts being well manipulated and squeezed, so as to induce the liquid carbolic acid, which was introduced, to penetrate into all the interstices of the wound. At a later period, in order to permit cicatrisation, the crust was clipped away around the margin, and a solution of sulphite of potash (5 grains to the ounce) applied.

In future, the method of introducing strong carbolic acid into wounds by means of a piece of lint soaked in the undiluted acid, held in dressing forceps, was adopted. The blood clots were as far as possible removed.

In order to obtain a more substantial crust and one less likely to be detached, in cases where there was too little blood, a paste was made use of, composed of starch, moistened with carbolic acid placed outside a piece of calico soaked in pure acid, and applied next the wound. As a rule, however, there is enough blood to form a substantial crust if several layers of calico are used.

A great risk of putrefaction was all along experienced, owing to the fact that the pure acid could not be made to overlap the skin around the wound because of the excoriation caused by it. This risk was especially great in the first twenty-four hours, during which there was a profuse flow of blood and serum. Hence attempts were made to obtain some sort of dressing containing the acid in a more diluted form, and the first-fruits of these attempts was the formation of various pastes, of which the chief was long known as carbolic putty. This consisted of a 1 in 5 solution of carbolic acid in boiled linseed oil mixed with common whiting ($CaCO_3$), to the consistence of a firm paste or putty. This was then spread on a sheet of block tin, forming a layer of about a quarter of an inch in thickness. A piece of lint dipped in 1–5 oily solution was retained permanently next the wound so as to prevent its exposure during the changing of the dressings. The whole dressing was firmly fixed down by means of a continuous series of strips of plaster, which, however, were absent at the most dependent part, so as to allow the escape of discharge, which was received on a towel. After some time it was found better to apply this putty between two layers of calico, and then the block tin outside all.

The advantages of this dressing are: the tin applied outside prevents the escape of the carbolic acid, the acid in the putty is just sufficiently diluted not to excoriate the skin, while the paste serves as a reservoir for the acid during the interval which elapses between the changing of the dressings, the discharge, as it flows out beneath the putty, taking up only a

certain amount of the acid in its course. If necessary the strength of the acid in the putty may be reduced.

Its disadvantages are that it is clumsy and inconvenient to manipulate, and that it is very apt, when subjected to movement, to crumble away, and thus become an uncertain dressing. Attempts were therefore made to improve it, and recourse was had to various forms of plasters. The first of these was the *lead plaster.*[1]

The following is the mode in which this is prepared :—

Take of olive oil	12 parts by measure	
,, ,, litharge (finely powdered)	12 parts by weight	
,, ,, beeswax	3 ,, ,, ,,	
,, ,, crystallised carbolic acid	2½ ,, ,, ,,	

Heat half the oil over a slow fire; then add the litharge gradually, stirring constantly till the mass becomes thick or a little stiff. Then add the other half of the oil, stirring the mass as before, till it becomes thick. Then add the wax gradually till the liquid again thickens. Remove from the fire and add the acid, stirring briskly till thoroughly mixed. Cover up close and set aside, and let the litharge settle, then pour off the fluid and spread on calico. The large quantity of litharge in this mixture was introduced for the purpose of preventing the melting of the plaster at the temperature of the body.

The wound was dressed with layers of lint soaked in carbolic oil, and this dressing was covered in all directions with the plaster. This plaster was renewed daily.

As with the putty, so here, the deep dressing was apt to become displaced, and therefore Mr. Lister used lead plaster as the outermost layer of the deep dressing, in the hope that it would adhere to the skin and prevent lifting of the edges of the dressing. In order to prevent the outer layer of the plaster from sticking to this inner portion, a piece of calico moistened in the watery solution was interposed. But though the plaster does very well for the outside layer, it does not do for the permanent dressing, for it allows the watery solution from the calico to soak through to the wound beneath. At the same time this deep layer, not having formed a crust, is apt to shift its place, and to leave the wound more or less exposed.

[1] *Brit. Med. Journal*, October 31, 1878.

Mr. Lister accordingly tried other sorts of plaster, and at length in shell lac he seemed to have found all that he wanted. This lac mixes in any proportion with carbolic acid, and is more or less fluid or tenacious, according to the quantity of acid present. The shell lac parts but slowly with its acid, and thus forms a good dressing. It is, however, very apt to stick, and in order to prevent this Mr. Lister interposed a layer of gutta-percha between it and the skin. The carbolic acid passes through the gutta-percha with extreme readiness, while the latter prevents the lac from sticking to the deeper parts. Gutta-percha tissue, however, proved apt to crack, and then the discharge got between it and the lac plaster, and thus the fluid beneath it received but little carbolic acid. Hence the same result happens with this cracked gutta-percha as when protective extends to the edge of the dressing—viz., a deep layer of the fluid is more or less protected from the action of the acid, and putrefaction can spread inwards.

After several experiments he ultimately employed a solution of gutta-percha in bisulphide of carbon to brush over the surface of the lac plaster.

The following is the method of preparing the plaster :—

'Take of shell lac	3 parts
,, ,, crystallised carbolic acid . . .	1 part

'Heat the lac with about one-third of the carbolic acid over a slow fire till the lac is completely melted, then remove from the fire and add the remainder of the acid, and stir briskly till the ingredients are thoroughly mixed. Strain through muslin and pour into the machine for spreading plaster, and when the liquid has thickened by cooling to a degree sufficient, spread to the thickness of one-fiftieth of an inch. Afterwards brush the surface of the plaster with a solution of gutta-percha in about 30 parts of bisulphide of carbon. When the bisulphide has all evaporated the plaster may be piled in suitable lengths in a tin box without adhering, or rolled up and kept in a canister.'

For the permanent dressing in compound fracture adhesiveness is wanted, and this is obtained by rubbing off the gutta-percha and brushing liquid carbolic acid over the surface.

At this time (1868) the necessity for protecting the healing parts from the direct action of the carbolic acid was beginning

to be recognised, and Mr. Lister first speaks of the use of block tin or sheet lead as a protective.

At the same time, having obtained a purer carbolic acid soluble in water, he gives up the application of the pure acid to the interior of the wound in compound fractures, and syringes it out with a solution of 1–20 instead.

In the 'British Medical Journal' for March 19, 1870, a description is given of the method then used. The wound was in the first instance thoroughly syringed out with 1–20 carbolic lotion. Then oil silk covered with a layer of dextrin was applied in one or two layers, and outside this the lac plaster. If much discharge was expected lint or a towel was placed outside the lac plaster in order to absorb the discharge. Where the dressing was left undisturbed for a week two layers of plaster were used. Where a small piece of lac plaster was applied as a deep dressing, and where consequently the gutta-percha was rubbed off to allow the plaster to adhere to the skin, the gutta-percha was left at one part so as to afford a channel for the escape of discharge.

In changing these dressings a solution of carbolic acid 1–20 was thrown over the wound by means of a syringe, as the dressing was raised, and then a guard soaked in the lotion was applied.

In 1869 catgut ligatures were introduced.

The objections to the lac plaster are that the discharge putrefies outside the plaster and irritates the skin, that the lac keeps the surface beneath it moist with what is really a film of watery solution of carbolic acid, and this again makes its way under the protective and irritates the wound.

Very shortly after the use of the aseptic method in the treatment of compound fractures it was applied to abscesses.

The first publication on this subject appeared in the 'Lancet' for July 27, 1867. The method of opening the abscess and of changing the dressings employed with the means at that time at disposal are very important with reference to the question of operating and dressing without a spray. A piece of rag dipped in 1–5 carbolic oil is laid on the skin where the incision is to be made. The lower edge of this rag being raised, a knife dipped in the oil is at once plunged into the abscess, and the rag immediately dropped over the orifice through which the pus is pressed out. A piece of lint

soaked in the oily solution is then introduced into the opening in order to stop bleeding and to prevent it from closing. This is done by slipping the strip of lint under the antiseptic rag. With regard to the dressing, the putty is here described and used outside a deep dressing of carbolic oiled lint, a layer of calico being interposed between the putty and the deep dressing to prevent them from sticking together. The putty is changed once in twenty-four hours, or oftener if necessary. In doing so a rag dipped in the oily solution is placed over the wound or the deep dressing the instant the old putty is removed. If a plug of lint has been introduced in the first instance it is after a time (two or three days) withdrawn by pushing oiled forceps under the piece of oiled rag, seizing hold of the plug and pulling it out, the oiled rag being pressed thoroughly around the forceps. If a probe is introduced it must be oiled and then slipped in between the folds of the oiled rag. As the various means described under compound fracture were introduced they were applied to abscesses on the principles already described.

The first example of efficient aseptic treatment in the case of wounds published by Mr. Lister will be found in 'The British Medical Journal' for October 31, 1868.[1] The case narrated is one of operation for badly united Pott's fracture.

'On the 11th inst. (April, 1868), the man being under the influence of chloroform, I made a curved incision behind and below the prominent end of the tibia, a solution of carbolic acid in four parts of olive oil being dropped into the wound during the progress of the operation. I detached the soft parts from the bone sufficiently to enable me to insinuate behind the callus one blade of a pair of cutting pliers smeared with the same oil, and then having placed pieces of lint soaked with the oil around the blades of the pliers, so as to prevent the chance of septic air entering the joint when the bone gave way, divided the callus, and at once covered the wound with the antiseptic lint. . . . The wounds were then dressed with a weak oily solution of carbolic acid, and covered with the antiseptic (lead) plaster. Fresh plaster was applied daily.'

I need not repeat the various modifications in the treatment

[1] Reports published during the same year by Drs. Heron and Malloch will be found in the *Glasgow Medical Journal*.

of wounds, as they are similar to those just described under compound fractures.

In the 'British Medical Journal' for January, 1871, Mr. Lister first mentions the gauze dressings, and also refers to the use of oakum. The spray is also tried. In August, 1871, this method has been established, the present protective was completed, and macintosh was substituted for gutta-percha tissue below the outer layer of the gauze. The drainage tube is first mentioned in August, 1871, in the treatment of wounds, though it had been used for a short time previously in the case of abscesses. At that time the piece of gauze next the wound was not wet. It was some two or three years later that the necessity for wetting the deep layer was recognised, and since that time the results have been much more constant. With the introduction of the steam spray, of the elastic bandage, of the improved gauze, &c., there has been a marked improvement in results, and the avoidance of putrefaction, in cases where the wound is made by the surgeon, is now reckoned on as a matter of course.

CHAPTER VII.

ASEPTIC SURGERY—(*concluded*).

Other methods of carrying out Aseptic Surgery. Substitutes for carbolic acid: *Salicylic acid: Thymol: Acetate of Alumina: Eucalyptus oil.* Aseptic surgery by filtration of the air. Subcutaneous surgery.

SUCH are the essential details of aseptic surgery as introduced and practised by Mr. Lister. The disadvantages arising from the irritating and poisonous qualities of carbolic acid have led some to seek other antiseptics as substitutes for carbolic acid. These attempts have not as yet, however, succeeded in producing any substance possessing so many advantages as that acid. The most successful substitute up till quite recently was salicylic acid, which is used on exactly the same principles, but not with the same constant aseptic results.

The use of salicylic acid was first advocated by Professor Thiersch, of Leipzig, and the following is a short abstract of his method of using it.

Salicylic acid is chemically nearly related to carbolic acid. Its formula is $C_7H_6O_3$, differing therefore from that of carbolic acid in containing in addition the atoms of carbonic anhydride. (The formula of carbolic acid is C_6H_6O.) Salicylic acid is not poisonous, but it affects the hands in the same way as carbolic acid. It is absorbed, and may be found in the urine of patients whose wounds are dressed with it.

A lotion of salicylic acid is employed. This is a saturated solution of the acid in water at the ordinary temperature, and its strength is about 1 part of salicylic acid to 300 parts of water.

Two materials are used as dressings—viz., salicylic wool and salicylic jute.

Salicylic wool is cotton wool impregnated with salicylic acid in the proportions of 3 and 10 per cent. by weight.

The 3 per cent. wool is made by dissolving 750 grammes of salicylic acid in 7,500 grammes of spirit (83 sp. gr.). This solution is then diluted with 150 litres of water at the temperature of 70°–80° C. 25 kilogrammes of pure cotton wool are saturated with this mixture.

The 10 per cent. wool is obtained by dissolving one kilogramme of salicylic acid in 10,000 grammes of spirit (83 sp. gr.), the solution being then mixed with 60 litres of water. Ten kilogrammes of pure cotton wool are soaked in this solution.

This soaking is best done in a large wooden vat, in which the layers of cotton wool have plenty of room. It is best to place only small quantities of wool (two to three kilogrammes) in this vat at a time in order to get an equal distribution of the acid. Thin layers of cotton wool are introduced into the salicylic solution under light pressure, fresh layers being added only when the former have been thoroughly soaked. When the whole quantity has been introduced the mass is turned over, so that the undermost layer becomes the uppermost, and then it is left for about ten minutes, so as to have equable distribution of the fluid. The wool is then taken from the vat and spread out in layers. On cooling, the acid crystallises out, and the layers are made up into small parcels, not exceeding two to three kilogrammes each. After twelve hours this wool is spread out to dry in a moderately warm place. It should not be hung up, lest the acid should become unequally distributed.

The 10 per cent. wool is coloured with carmine for the sake of distinction.

It is important to note that Thiersch, in speaking of 3 per cent. and 10 per cent. wool, means wool soaked in the solutions of the strength described. The wool does not contain that per centage of salicylic acid.

This cotton wool does not absorb fluids readily, and therefore Thiersch now uses jute. This is made from the bark of various species of Corchorus grown in Bengal, and is cheaper than cotton wool, and at the same time more absorbent. It is used of two strengths—3 and 10 per cent. prepared in the same way as the salicylic wool.

Glycerine is added to the solution in order to prevent the crystals of salicylic acid from falling out, because they are apt to produce violent sneezing, coughing, &c.

In order to obtain the 3 per cent. jute 2,500 grammes of jute are put into a solution of 75 grammes of salicylic acid, in 500 grammes of glycerine and 4,500 grammes of water at 70°–80° C.

In the glycerine jute the acid is more equally distributed than in the cotton wool. In the case of the latter the cotton is frequently so imperfectly charged that it is necessary to place a layer of 10 per cent. cotton next the wound, and then outside this the 3 per cent. wool. In the case of the glycerine jute a 4 per cent. material is sufficient for the whole dressing.

As to the spray Thiersch does not care whether it is 1–50 carbolic acid or 1–300 salicylic acid. Carbolic acid is to be preferred, because it causes less coughing and sneezing, and it does not adhere to the clothes.

Salicylic acid is best in some cases, as it irritates the wound less than the carbolic.

For disinfecting the hands and skin, carbolic acid or salicylic acid may be used, but for the instruments carbolic acid must be employed, because the steel becomes oxidised in a solution of salicylic acid.

The sponges are washed in carbolic acid.

No protective is required, because the salicylic acid is but little irritating.

Macintosh is also unnecessary.

In order to enable the dressing to peel off and to let the discharge get away more easily a layer of gutta-percha tissue or of oiled silk riddled with holes and covered with a piece of gauze is applied next the wound.

This treatment may be illustrated by a case of amputation.

The patient having been chloroformed and Esmarch's elastic bandage applied, the part is shaved, washed with soap and water, spirit and turpentine oil, and then with salicylic acid lotion, or with the 1–20 carbolic acid solution. It is also scrubbed with a nail-brush for a few minutes (quite unnecessary). The operation is carried out with the usual aseptic precautions. After arresting the hæmorrhage the wound is

closed with deep and superficial stitches. A drainage tube is then introduced into each angle, and the wound is washed out with salicylic acid solution till the fluid which comes out is clear (unnecessary). Three finger-breadths of perforated gutta-percha and of carbolic gauze is then applied; over this one finger's thickness of the strong salicylic wool, and outside this two fingers' thickness of the weak wool. The whole is then fastened on with a bandage.

If the patient complains of pain the dressing is changed and the wound examined. If not, it is left till the eighth or tenth day, when it is changed, in order to remove the drainage tube. If any discharge comes through in the first instance fresh wool is put outside the dressing. The second dressing is left till healing is complete.

Large compound fractures are treated at first by irrigation with salicylic acid. In order to protect the skin from maceration by the fluid it is from time to time rubbed with palm oil. After all risk of abscess formation has passed off and the wound is granulating well, one may apply dry salicylic dressing as before described.

Where there is a tendency to inflammation, more especially where there is imperfect drainage with progressive abscess formation, wet salicylic dressing should be applied. This is ordinary salicylic dressing, which is from time to time soaked with salicylic lotion.

Thymol as an antiseptic application to wounds was introduced some years ago by Ranke of Halle, and was much lauded on account of its non-poisonous and non-irritating qualities.

The thymol gauze was made on the same principles as the carbolic gauze, spermaceti being, however, employed. A thymol solution of the strength of 1–1000 is made by the addition of alcohol and glycerine.

This antiseptic has not answered the expectations entertained at first. It does not prevent putrefaction, and has been justly abandoned in aseptic work.

Acetate of alumina has been lately used by Maas. He applies lint dipped in the solution ($2\frac{1}{2}$ per cent.) to the wounds,

and covers this with macintosh. The strength of the spray is also $2\frac{1}{2}$ per cent.

He says that this is a powerful antiseptic, and that with it he gets typical aseptic results. The substance is unirritating, and very few dressings are required.

Eucalyptus oil has recently been strongly advocated by Dr. Schulz of Bonn.[1] Its antiseptic properties were shown by Bucholtz in his paper on antiseptics. He found that it was three times as strong as carbolic acid, for while carbolic acid prevented putrefaction when present in the proportion of 1 in 200 parts, the eucalyptus oil only required to be present in the proportion of 1 to 666·6 parts to produce the same effect.

Siegen also showed that eucalyptus oil prevents putrefaction and alcoholic fermentation better than carbolic acid. He found that blood to which $\frac{1}{3}$ per cent. of eucalyptus oil had been added was quite odourless ten days later. Bing states further that it hinders the passage of white corpuscles out of the vessels, and that therefore, on Cohnheim's theory, it is an agent capable of arresting suppuration.

With regard to its usefulness, its smell is more pleasant than that of carbolic acid. It dissolves readily in alcohol or in oil, and mixes perfectly with pure paraffin.

Schulz also states from Siegen's experiments and from his own that the eucalyptus oil is not poisonous. The tree from which the oil is obtained grows in large numbers in Australia, and the oil can be obtained in large quantities and very cheap.

Schulz recommends that for the spray the glass bottle should be filled with the pure oil or with oil dissolved in alcohol. The steam would then pick this up and make an emulsion.

As a lotion it might be used in the form of an emulsion.

Schulz proposes that the wounds should be dressed with lint saturated with a 10 per cent. solution of eucalyptus or olive oil. Outside this, or instead of it, may be used Lister's gauze dressing containing eucalyptus oil instead of carbolic acid. A gauze which contains even 50 per cent. of eucalyptus oil may be made with paraffin. Dr. Schulz has not himself, however, used this method.

[1] *Centralblatt für Chirurgie*, January 21, 1880.

Quite recently Mr. Lister has been making an extensive trial of eucalyptus oil in the treatment of wounds. A gauze has been prepared similar to the ordinary gauze, but containing eucalyptus oil instead of carbolic acid. Dammar has also been substituted for the ordinary resin. So far this has proved very satisfactory. It seems to be trustworthy as an antiseptic, and can be used under circumstances where carbolic acid is apt to cause irritation, as in dressings on the scrotum, or in patients whose skins are liable to be irritated by carbolic acid. Being non-poisonous, it may also be substituted for carbolic acid in cases where constitutional effects are apt to follow the absorption of the latter.

It has also been employed as an ointment in the proportion of 1 part by measure of the oil to 4 parts by weight of the same base as is used for the boracic and salicylic ointments (p. 65). This ointment is employed in the cases for which boracic and salicylic ointments have up to the present been used, and it possesses the advantage over the latter in that the oil not only renders the discharge pure as it passes over it, but also on account of its volatility bathes the parts in an antiseptic vapour. Hence it will probably be especially useful in the treatment of burns (see p. 116). Its non-poisonous qualities are also a great point. As yet no experiments have been made with the view of substituting it for carbolic acid in the lotions used in the spray, in washing wounds, purifying instruments, &c., and indeed the whole matter is only for the present under trial.

So far we have been considering modes of preventing putrefaction in wounds based on the fact that the septic particles in the air and on surrounding objects may be deprived of their power of causing fermentation by contact with some suitable chemical substance. But we also saw in the preliminary considerations that it sufficed for the avoidance of fermentation in flasks to keep the dust out mechanically, as, for instance, by means of cotton wool. This fact was made use of by Mr. Lister some years ago in the following manner. I may quote his remarks, which are given in a foot-note to his article on Amputations in Holmes' 'Surgery,' vol. v. p. 619, published in 1871.

'Among recent contributions of fact to the elucidation of this question (the germ theory) may be mentioned Professor Tyndall's simple but beautiful proof of the existence of organic particles of dust of excessive minuteness in the air by means of a condensed beam of light, and the equally clear ocular demonstration afforded by the same method, that even the finest particles are capable of being removed from the air by causes which Pasteur, in some of his experiments, inferred must clear it of suspended organisms, such as the action of gravity and filtration by cotton wool. The fact last named seemed to promise valuable results in antiseptic surgery, and experiments made with this view have afforded further evidence in favour of the germ theory which it may be well to mention here. I found that if cotton wool impregnated with either chlorine or sulphurous acid gas or with the vapour of benzine or carbolic acid, was placed upon a wound or granulating sore, after washing the surface with a solution containing the same agent, *although the volatile antiseptic left the cotton in about a day*, the blood or pus still effused beneath the cotton remained free from putrefaction for an indefinite time, provided that the discharge was not sufficiently copious to soak through the cotton and appear at the surface, in which case the meshes between the fibres affording ample space for microscopic organisms to develop in, putrefaction spread within a few hours throughout the moistened part of the mass. This circumstance greatly interfered with the practical utility of the dressing, and it has since been superseded by the antiseptic gauze to be described in the text, but the facts seem to me important with regard to the germ theory. The cotton wool, though it loses all chemical antiseptic virtue in a day, yet will keep out putrefaction for a month or more. It cannot possibly keep out any atmospheric gas, which is necessarily diffused freely between its fibres, and gets in for the same reason that the volatile antiseptic gets out. That which it does exclude can only be suspended particles of dust. It follows, therefore, as a matter of certainty, that the cause of putrefaction through atmospheric influence of blood or pus, or, in other words, such materials as the surgeon has to deal with in treating wounds,

are not the atmospheric gases, but dust, and the fact that this dust is deprived of its putrefactive energy by agents which are chemically so unlike as chlorine, sulphurous acid, benzine, and carbolic acid, but which agree in having a common hostility to animal or vegetable life (I used benzine because I knew that the entomologist employs its vapour to kill insects), this fact confirms the view that the putrefactive particles are really organisms. I commend these simple experiments with cotton wool to the candid judgment of the reader, because, whatever may be thought of their bearing upon the allied subject of spontaneous generation, they must be allowed to afford absolute demonstration of the truth which is the foundation of the antiseptic system, viz., that the putrefaction of blood or pus under atmospheric influences is caused not by the gases of the air, but by suspended particles, which can be deprived entirely of their septic energy by the vapour of an agent like carbolic acid.'

It will thus be seen, that what Mr. Lister used here was not an antiseptic application but an aseptic one, and that the only mode in which this dressing acted was by mechanically preventing the particles from reaching the wound. For the reasons quoted, this method has not been turned to practical account, though, as we have seen, it is still used in cases of gangrene in order to protect the weak parts from all sources of irritation.

Mr. Barker, assistant surgeon to University College Hospital, has tried a similar method in one or two cases. He purified cotton wool by heat, and applied between it and the wound a layer of lint dipped in carbolic oil. This method, however, seems to be impracticable, for after the wool has been heated, but before it is applied, dust would very probably gain access to it unless very complicated precautions were taken. I believe that if pure cotton wool is used Mr. Lister's method is the only practicable one.

I have mentioned these experiments more as confirming the flask experiments described before than for the purpose of recommending the method for adoption. I believe that thoroughly satisfactory and indeed the best results may be obtained by the use of suitable chemical means.

Subcutaneous surgery is another way in which the aseptic principle may be carried out. Here the wound is made under the skin and away from the air dust altogether. This method will be better understood when we come to trace the history of antiseptic surgery.

CHAPTER VIII.

THEORIES OF SPONTANEOUS GENERATION, HETEROGENESIS AND ABIOGENESIS.

Principles on which other methods of antiseptic surgery act. Organisms are always present in fermenting liquids: their significance. Theory of the origin of organisms independently of a parent. Theories of Needham and Buffon: Needham's proofs. Spallanzani's experiments: Needham's objections: Spallanzani's replies. Schulze's experiments. Schwann—Schroeder and Dusch—Schroeder—Doctrine of Heterogenesis. Pouchet's work: his method of testing the matter: proofs that the source of the organisms in infusions is neither the air, water, nor the putrescible substances: modes of repeating Schulze's and Schwann's experiments: examination of dust. Criticism of his results. Pasteur's experiments: results with ordinary fluids: introduction of dust into sterilised fluids: results with milk and alkaline fluids: the cause in the air which gives rise to the growth of organisms is particulate: Pasteur's cultivating fluid: estimate of Pasteur's work. Pouchet's reply: New experiments—Criticism of these.

Such are the methods by which that form of antiseptic surgery which aims at the total *exclusion* of septic ferments may be best carried out. But 'Antiseptic Surgery' in its broad sense includes another class of methods of treatment acting on a totally distinct principle, and interfering more or less perfectly with the occurrence of fermentations. These all act on the principle of *rendering inert the causes of putrefaction after their entrance into the wound, of offering obstacles, more or less complete to the fermentation which these particles would otherwise occasion.* In order to understand these methods, to see on what principles they act, to decide which are the best, and to carry them out with the greatest success, it is necessary to take up our discussion of the causes of putrefaction at the point which we have already reached, and to consider what is *the nature of the particles* which we found to be the causes of

fermentative changes, and how it is that they bring about these changes.

As is well known, micro-organisms of some form or other (bacteria, torulæ, &c.) are always present in fermenting liquids, and the view which is now almost universally held by scientific men is that these bodies are the initiators of the chemical change.

We have already seen that fermentation occurs only after the access of particles from the outer world, and it is asserted by the supporters of the germ theory of fermentative changes, that these particles are organisms or their spores, and that it is by the growth of these organisms in the fermentescible material that the latter undergoes alteration.

Some, however, assert that these organisms are only accidental accompaniments of the process of fermentation ; in fact a few still maintain that they arise in fermenting substances from agglomeration of the molecules of that material, that in fact they are generated anew and are not necessarily derived from a parent. It is therefore necessary for us, before discussing the germ theory of fermentation, to consider what are the real facts with regard to this matter of abiogenesis.

The first views of which we must take notice, as being the first founded on experiment and observation apart from mere philosophical speculation, are those of Needham and Buffon, published in the middle of the eighteenth century.[1] Needham's

[1] It may be of interest to quote Needham's own words somewhat in detail. Referring to Spallanzani's criticisms of his work (*Nouvelles Recherches sur les Êtres microscopiques*, by Spallanzani, translated by M. l'Abbé Regley, 1769), he says (vol. i. p. 142) : 'Il (Spallanzani) sait très-bien par toute la teneur de mes observations microscopiques que je ne donne aucune autre puissance à la matière que celle qui produit la pure vitalité dénuée de toute sensation, et qui dérive, comme son existence primitive, de la seule Divinité ; que cette vitalité est un composé matériel de la force résistante et de la force expansive, dont les premiers principes ont été donnés à la matière par le Créateur au moment de la création : que tout corps, ou partie organisée, est une procession ou prolongation d'un corps organisé, soit végétal ou animal, qui doit nécessairement préexister, et dont la souche primitive sort immédiatement des mains de Dieu ; que cette procession ou prolongation insensible, que doit donner ce germe nouveau, dont la petitesse est indéfinie, pour se conformer à toutes les circonstances possibles, se fait moyennant une espèce de réduction dirigée par les forces plastiques, et une concentration des parties spécifiques, qui tendent, en les atténuant, vers un point déterminé ou un certain foyer commun, de même à peu près que l'œil est au monde visible, un centre où les

theory was, that there is in matter a force charged with the formation and government of the organic world, which force he

rayons viennent s'arranger de toutes parts, sans confusion, dans le même ordre qu'ils reçoivent de l'harmonie préétablie de l'univers ; que quant aux premiers principes de cette vitalité purement matérielle, il y a une matière indubitablement démontrée par des expériences constantes, très-atténuée, très-exaltée, éthérée selon Newton, électrique selon les idées présentes, très-élastique par sa nature intime, toujours prête à donner le branle à la matière brute et résistante, et qui pénètre substantiellement la masse entière ; que par conséquent ces deux especes de matière, mêlées dans toutes les proportions possibles, peuvent fournir les témperaments nécessaires pour tout degré de vitalité quelconque, et pour tous les grands phénomènes ou changements physiques de l'univers en partant d'un seul principe ; que cette vitalité, n'étant autre chose qu'un esprit très-subtil et très-actif, agissant dans une matière brute, tenace et ductile, pour former, selon les forces spécifiques de chaque corps vital, un nouveau système organisé, est très-différente, selon mes idées, du principe sensitif, qui ne peut être composé, et encore plus distinguée du principe intellectuel et spirituel, l'âme de l'homme.'

Further on (p. 150) he reiterates his view that these beings are distinct from the higher classes of animals which possess sensation. 'En général, toute substance quelconque, animale ou végétale, se décompose, selon moi, en êtres que j'appelle *vitaux* pour les distinguer des animaux parfaits à qui la Divinité a ajouté par surcroît les puissances purement sensitives, ou sensitives-intellectuelles.'

Again (p. 172), 'L'auteur (Spallanzani) croit que j'ai parlé de la force ordinaire végétante des plantes, par laquelle elles développent en feuilles, en branches et en racines. Il n'est rien de tout cela. Quand il s'agit de la production de ces corps organiques, je considère au contraire la plante dans un état de corruption comme plante : car c'est alors qu'elle perd absolument sa forme primitive et, qu'après avoir été dépouillée de ses sels, de ses huiles, et des autres principes constitutifs, ce qui reste devient une matière gélatineuse et toute filamenteuse qui végète par elle-même en branches vitales et se partage en corps ronds animés ou pousse au-dehors des globules mouvants.'

'Voilà en peu de mots le vrai tableau, voilà le raisonnement de M. de Buffon et le mien. Il y a certainement un principe de vitalité matériel distingué du principe sensitif, seul constitutif de la stricte animalité, qui se dispose organiquement, et qui, subordonné aux lois générales établies par la Divinité, végète dans les corps animaux qu'il forme comme dans les végétaux, en les animant à la façon ordinaire' (p. 166).

'Ce principe de vitalité est le *seul* principe d'économie et d'action dans les végétaux, et dans une certaine classe de ces êtres qui, paraissant sensitifs sans l'être, servent à lier ensemble le végétal et l'animal sensitif' (p. 166).

'Mais j'ai toujours reconnu comme nécessaire pour compléter le vrai animal, qui doit être sensitif, un principe de sensation, une âme qui n'est pas composée comme le système organique, et qui, quoique anéantie avec le corps selon le bon plaisir de son créateur, est néanmoins supérieure à la vitalité, et hors de toutes les puissances de la matière la plus exaltée.'

Buffon (*Histoire Naturelle*, vol. ii. p. 420, 1749) says : 'Tous les animaux

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calls *force végétatrice.* He imagines that this force, by setting into motion all the particles of matter, excites in some of them a sort of vitality distinct from sensation, and produced by the union of two other forces, which he terms *force résistance* and *force expansive.*

The proofs on which Needham bases his views as to the spontaneous origin of these minute organisms are of three distinct kinds.

The first discussing the different phenomena furnished by different infusions, more especially the enormous variety of forms arising in them ; and the second alluding to the behaviour of infusions after being subjected to heat, dependent as these arguments are on microscopical examination, need not be considered here, because the construction of the microscope was at that time so imperfect as to make it of little or no use for such observations.

The third, and indeed the only experiments which require to be noticed, are those in which infusions, contained in vessels hermetically sealed, are subjected to the action of heat for a prolonged period. In infusions treated in this way by Need-

se nourrissent de végétaux ou d'autres animaux, qui se nourrissent eux-mêmes de végétaux ; il y a donc dans la nature une matière commune aux uns et aux autres qui sert à la nutrition et au développement de tout ce qui vit ou végète; cette matière ne peut opérer la nutrition et le développement qu'en s'assimilant à chaque partie du corps de l'animal ou du végétal, et en pénétrant intimement la forme de ces parties, que j'ai appelée le moule intérieur. Lorsque cette matière nutritive est plus abondante qu'il ne faut pour nourrir et développer le corps animal ou végétal, elle est renvoyée de toutes les parties du corps dans un ou dans plusieurs réservoirs sous la forme d'une liqueur ; cette liqueur contient toutes les molécules analogues au corps de l'animal, et par conséquent tout ce qui est nécessaire à la reproduction d'un petit être entièrement semblable au premier. Ordinairement cette matière nutritive ne devient surabondante, dans le plus grand nombre des espèces d'animaux, que quand le corps a pris la plus grande partie de son accroissement, et c'est par cette raison que les animaux ne sont en état d'engendrer que dans ce temps.

'Lorsque cette matière nutritive et productive, qui est universellement répandue, a passé par le moule intérieur de l'animal ou du végétal, et qu'elle trouve une matrice convenable, elle produit un animal ou un végétal de même espèce ; mais lorsqu'elle ne se trouve pas dans une matrice convenable. elle produit des êtres organisés différents des animaux et des végétaux, comme les corps mouvans et végétants que l'on voit dans les liqueurs séminales des animaux, dans les infusions des germes des plantes, &c.'

ham and boiled for many minutes organisms developed very readily.

Spallanzani[1] repeated these experiments, and he found that though some infusions could be sterilised after boiling for a short time, yet it was necessary to keep others at the boiling temperature for an hour or more before they would remain permanently sterile. Spallanzani's method was to heat his flasks, then to pour in the liquid, hermetically seal the flask, and place it in a water bath. The error in this method is probably that the impure fluid when poured into the flask, soiled the neck which he had previously purified by heat.

To these experiments Needham objected that Spallanzani had much enfeebled or perhaps destroyed the *force végétatrice* of the infusions by keeping them exposed to the action of heat for so long a period of time as an hour.

This objection was at once met by Spallanzani, who showed that organisms rapidly developed in these same infusions, if they were left exposed to the air after this prolonged boiling; and he truly says that, if the organisms only come from the fluid, and if the power which this possesses of generating these beings is destroyed by heat, they would remain absent whether the flasks were open or shut. He even went further and heated the vegetables very strongly before infusing them, but even in this case organisms developed in the fluids so prepared.

Needham, however, said that the small quantity of air remaining in the flasks was completely altered by the exhalations from the fluid and by the heat of the fire, and that thus the *force végétatrice* could not act. This objection cannot be said to have been in any way met by Spallanzani. In some cases, indeed, he succeeded in preventing the appearance of organisms by boiling the fluid from a half to two minutes, but in many cases minute organisms appeared. In order to prevent their occurrence in all instances, it was necessary to prolong the heat for at least three-quarters of an hour.

As an answer to Needham's last objection the experiments of Schulze[2] form a most important step in advance. The

[1] *Opuscules de Physique animale et végétale*; traduits par Jean Senebier, Genève, 1777.

[2] See translation in *Microscopical Journal*, 1841.

following was the problem which he proposed and the method adopted to solve it.

Query: 'If the access of atmospheric air, light, and heat to substances in flasks included of itself all the conditions for the primary formation of animal or vegetable organisms? The difficulties to be overcome consist in the necessity of being assured first, that at the beginning of the experiments there was no animal germ capable of development present in the infusion, and secondly, that the air admitted contained nothing of the kind.'

His method of procedure is described by himself as follows :—

'I filled a glass flask half full of distilled water, in which I mixed various animal and vegetable substances. I then closed it with a

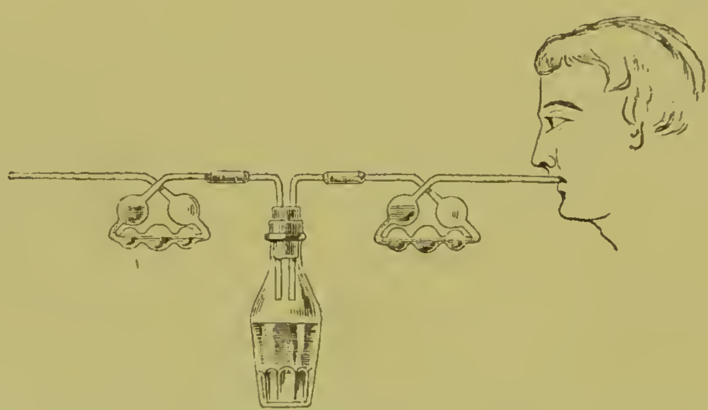


FIG. 58.—SCHULZE'S METHOD OF DEMONSTRATING THAT ORGANISMS ARE DERIVED FROM THE AIR AND DO NOT ORIGINATE SPONTANEOUSLY IN LIQUIDS.

good cork, through which I passed two glass tubes, bent at right angles, the whole being air-tight. It was next placed in a sand-bath and heated until the water boiled violently, and thus all parts had reached the temperature of 212° F.

'While the watery vapour was escaping by the glass tubes I fastened at each end an apparatus which chemists employ for collecting carbonic acid; that to the left was filled with concentrated sulphuric acid, and the other with a solution of potash. By means of the boiling heat everything living and all germs in the flask or in the tubes were destroyed, and all access was cut off by the sulphuric acid on the one side and by the potash on the other. I placed this easily moved apparatus before my window, where it was exposed to the action of light, and also, as I performed my experiments during

the summer, to that of heat. At the same time I placed near it an open vessel with the same substances that had been introduced into the flask, and also after having subjected them to the boiling temperature. In order now to renew constantly the air within the flask, I sucked with my mouth several times a day the open end of the apparatus filled with solution of potash; by which process the air entered my mouth from the flask through the caustic liquid, and the atmospheric air entered the flask from without through the sulphuric acid (Fig. 58). The air was of course not at all altered in its composition by passing through the sulphuric acid in the flask, but if sufficient time was allowed for the passage, all the portions of living matter or of matter capable of becoming animated, were taken up by the acid and destroyed. From May 28 till the beginning of August I continued uninterruptedly the renewal of the air in the flask, without being able, by the aid of the microscope (magnifying glass?), to perceive any living animal or vegetable substance, although during the whole of the time I made my observations almost daily on the edge of the liquid; and when at last I separated the different parts of the apparatus I could not find in the whole liquid the slightest trace of infusoria, of confervæ, or of mould. But all the three presented themselves in great abundance a few days after I had left the flask standing open. The vessel which I placed near the apparatus contained on the following day vibriones and monads, to which were soon added larger Polygastric Infusoria, and afterwards Rotatoria.'

By these experiments the fears entertained by Needham as to an alteration in the air contained in the flask being the cause of the sterility of the infusion, were completely set at rest; for here air, which had not been subjected to heat, and which was constantly changed, was present in the vessel in which no development occurred, while the second open vessel showed that the power of the liquid to nourish organisms had not been lost by boiling. It was therefore clear that in this instance the organisms which grew in the outer vessel came in some way or other from particles in the atmosphere, which could be destroyed by sulphuric acid. Whether or not both modes of origin might not exist, and whether the bodies falling into the fluid from the atmosphere were organisms or their spores, or merely albuminous matters which gave rise to organisms, was as yet in no way determined.

And the results of the experiments of Schwann, mentioned

before, leave us in the same position. In his case the heating of the air was substituted for Schulze's method of passing it through a chemical substance, but no further evidence was obtained. Advance was no doubt made by his results in that the objection which might have been urged by some against Schulze's experiments, viz., that particles of the sulphuric acid were carried over with the air, or that the air was in some way or other altered, are entirely removed,[1] while at the same time it was then known that heating air produced no alteration in the gases of the air. Schwann himself explained his results by supposing that the spores of infusoria and other smaller organisms are present in air, and are destroyed by heat; and he held that putrefaction and other fermentative changes are brought about by these organisms abstracting materials for their nutrition from the fluids in which they grow, and leaving the compounds thus broken up to form new combinations.

One most important fact he does mention, viz., that blood can be received into and preserved in a flask with certain precautions, without the development in it of any form of life.

Further evidence with regard to boiled substances was brought forward by Schroeder and Dusch, and later by Schroeder alone. Their method of experimentation by filtration of the air through cotton wool, and their results, have been already referred to (p. 12, *et seq.*), and it will be sufficient to add here that in those cases where putrefaction occurred, organisms were present, while in the flasks, in which no change took place, organisms were absent. By their method, meat, meat infusion, and malt were preserved after boiling without any appearance of organisms.

Difficulties were experienced with milk and yolk of egg, but these were finally overcome either by heating them to the temperature of 130° C., or by prolonged heat at 100° C. These experiments are of the greatest importance, as with the method

[1] This objection that the air is altered in passing through sulphuric acid is not urged by Pouchet, who indeed states that no alteration occurs, and that organisms can develop as readily when such air is admitted as in presence of ordinary air. Schulze's experiment also proves that the barrenness of the liquid was not due to the entrance of sulphuric acid into it, for the liquid became full of life after having been exposed for a few days to ordinary air.

employed no objections can be raised as to any alteration in composition of the air.

When, then, we look at Needham's two objections, which have been urged against experiments with boiled fluids, and when we compare with them the answers furnished by all observers, but more especially by Spallanzani to the first, and the progressively strong replies to the second by Schulze, Schwann, and Schroeder and Dusch, we must, I think, come to the conclusion that they have been completely met. Hence in order to retain the theory of spontaneous generation it became necessary for the heterogenists to change their ground. They had to admit that there were present in the air and on surrounding objects particles (not necessarily bacteria or their germs, though very probably so) which, falling on suitable soil, gave rise to the development of bacteria; but they still held that under certain circumstances heterogenesis may also occur, though possibly more rarely than propagation from a parent.

They attempted to support this view in two ways: firstly, by denying the accuracy of the former experiments, by pointing out that they do not always succeed, and that organisms develop in some materials, even after prolonged exposure to a high temperature; and secondly, by reference to the results of attempts to preserve unboiled fluids and tissues.

In 1859 there appeared the work of one of the most ardent supporters of the theory of spontaneous generation—Pouchet—and it is necessary for us to examine his views and facts somewhat in detail.[1]

Pouchet does not look on these organisms as originating from dead matter through the action of some mysterious force, as has been since advanced by some heterogenists. Their sources are, according to him: 'Des particules organiques, débris des anciennes générations d'animaux et de plantes, qui se trouvent combinées aux parties constituantes des minéraux. Selon cette doctrine ce ne sont donc pas des molécules minérales qui s'organisent, mais bien des particules organiques qui sont appelées à une nouvelle vie.' He further states that though he believes that it is the contact of different bodies which gives rise to the

[1] *Hétérogénie ou Traité de la Génération spontanée basé sur des nouvelles expériences.* Paris, 1859.

development of proto-organisms, yet he does not think that their origin is due to affinity alone—vital force must also come into play. This vital force owes its manifestation to certain unknown concomitant circumstances; thus fermentative or catalytic phenomena precede all spontaneous generation. In connection with these views he describes the development of ova in what he terms the proligerous pellicle, or scum on the surface of fermenting fluids. With regard to this he says: 'La génération primaire ne produit jamais un animal de toutes pièces, mais seulement elle engendre des ovules spontanés dans le milieu proligère absolument sous l'empire des mêmes forces qui façonnent des ovules dans le tissu de l'ovaire.'

The essentials for the production of new forms are, according to him, a putrescible body, water, and air, while heat, light, and electricity considerably favour the result. Having shown that the first three are essential, though they need not necessarily be present in large amount, he proceeds to state the problem in a very fallacious manner: 'Si l'on admet,' says he, 'que dans nos expériences la génération ne peut s'opérer qu'à l'aide de trois facteurs, et que c'est l'un d'eux seul qui récèle les germes des proto-organismes, il est évident que si l'on prend chacun de ces trois corps en particulier, sans s'inquiéter nullement alors des deux autres, et que l'on démontre successivement que ce n'est aucun d'eux qui contient ces germes, il faudra bien, en somme, reconnaître quand le fait aura été strictement établi pour chacun isolément, que ce n'est donc aucun de ces trois corps qui peut servir d'asile aux œufs ou aux séminales introuvables des êtres divers qu'on voit s'engendrer sous les yeux.'

He tests the question in the following manner: 1. With regard to the first point he says that it is evident that the putrescible material does not contain the germs of the proto-organisms since, even though it is charred previously to its employment, the water in which it is placed becomes rapidly filled with microzoaires and cryptogams.

Experiment.—10 grammes of any of the following seeds, maize, peas, beans or lentils, were placed in an iron spoon and completely charred; then the product was placed in a glass vessel containing 500 grammes of distilled water and covered with a bell jar. In twenty days the fluids were found to contain micro-organisms and cryptogams.

2. He next points out that it is not the water which contains the germs because, if one places organic substances in an artificial water, animalculæ and cryptogams still develop.

To show this an artificial water was obtained in the following manner :—'Into a large flask with two orifices, water and fragments of zinc are placed : one of the orifices transmits a tube terminating in a funnel by means of which sulphuric acid may be introduced into the flask ; the other orifice is attached to a large horizontal tube filled with asbestos, which leads to a small tube drawn out at its extremity, and terminating close to the outside of a metal vessel filled with cold water. The sulphuric acid having been introduced, hydrogen gas is disengaged, and is lit at its exit from the tube. The flame being close to the metal vessel, moistens its walls with watery vapour, the result of the combination of the oxygen of the air with the hydrogen from the apparatus: and this vapour, being condensed, is caught in a platinum vessel. The apparatus having been maintained in this state for three days, 200 grammes of water were obtained, and were employed in the two following comparative experiments.'

Half of this water was boiled for a quarter of an hour, in order to kill the germs which might have fallen into it, although he does not consider that to be necessary. This water was then introduced into a vessel with 5 grammes of hay, which had been raised to a temperature of 200° C. The vessel was placed in a basin containing a little water, and the whole was covered with a small shade. In four days there was a granular pellicle and two species of Paramecia.

The other portion of distilled water was not boiled, and to it was added hay which had not been heated; the result was absolutely identical.

3. Lastly, Pouchet states that it is evidently not the atmosphere which disseminates the germs, since he has seen organisms appear in flasks containing only artificial air, or in flasks containing air which had been heated or which had passed through sulphuric acid.

He next takes up Schulze's experiment, and presents the following as a counter experiment and as a complete disproof of the former :—

A vessel of the capacity of one litre was half filled with water, to which 5 grammes of hay were added. The cork of the vessel was traversed by two tubes each bent at right angles, five centimetres above the place of exit; one, the afferent, did not descend into the interior

of the vessel lower than its neck, the other, the efferent, reached to within one centimetre of the liquid, in order the better to remove the heavy stagnant gases. To each of the tubes was attached Liebig's bulbs filled with sulphuric acid. To the efferent was further attached

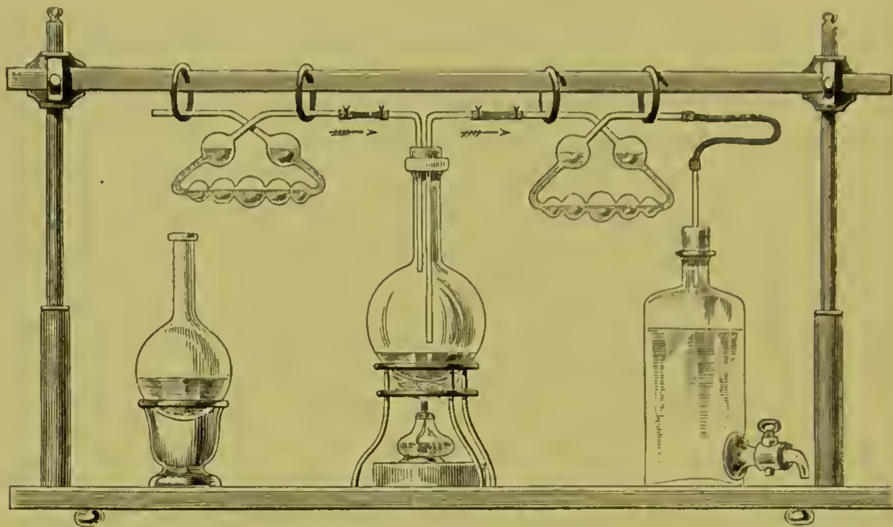


FIG. 59.—POUCHET'S MODE OF REPEATING SCHULZE'S EXPERIMENT. (FROM POUCHET).

a water aspirator, which was kept constantly acting (Fig. 59). The cork, &c., were all luted with copal. The fluid was then boiled for an hour. In twenty-six days spirilla, vibriones, and penicillium were present in the fluid.

In order, as he supposes, to have more rigorous conditions than those of Schulze, he introduces the following method of procedure:

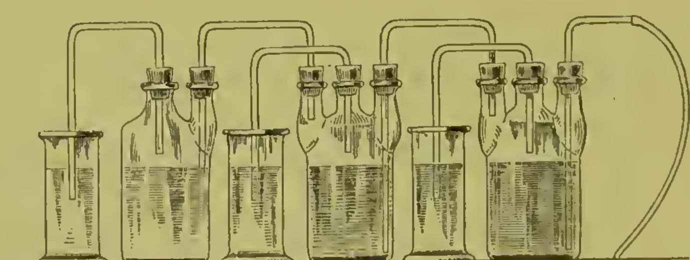


FIG. 60.—ANOTHER MODE ADOPTED BY POUCHET FOR TESTING SCHULZE'S VIEWS (FROM POUCHET).

A flask is used having the capacity of one litre, and having three necks. This is quite filled with concentrated sulphuric acid. The first neck contains a bent tube, which by one extremity communicates with an air pump, while the other extremity passes down to the

bottom of the acid; through the middle neck passes a siphon, which commences in the flask at the level of union of the upper and middle thirds, while its other end passes into an empty vessel. The third orifice contains a tube passing down to the bottom of a second vessel, but only originating in the first vessel at the very top. This second vessel, of the same capacity as the other, had been filled with boiling water; the first neck allowed the passage of the tube from the first vessel; through the central one passed a siphon similarly arranged to that in the first vessel: while from the third opening a tube led into a third vessel. This third vessel had the same capacity as the others, but had only two openings, the first receiving the tube from the second flask, which, however, did not pass down to the bottom of the third flask as in the other two cases; and the second giving exit to a siphon arranged as before (Fig. 60).

A strong decoction of boiling hay having been introduced into the third vessel so as to fill it exactly, the whole apparatus was luted with the greatest care, and thus the apparatus contained only this decoction, the sulphuric acid and the water which had been previously boiled. Air was then forced slowly into the sulphuric acid, and traversing the acid, it collected at the top, forcing the sulphuric acid through the siphon; this air then passed on through the second flask, passing through the water and forcing the water through the siphon; and from the second into the third vessel, forcing some of the decoction through the siphon; then the apparatus was abandoned to itself, and a quantity of the same decoction was placed outside for comparison. In twenty days the hay infusion, both in the apparatus and in the vessel outside, contained 'mucor' and bacteria.

A second experiment was done by introducing into the third flask 5 grammes of hay, which had been exposed to the action of steam for five minutes. The vessel was then filled with boiling water. A similar result was obtained.

Again, he introduced into the third flask filtered water, and 10 grammes of hay exposed for thirty minutes to a temperature of 200° C. Nevertheless, monads and vibriones were found.

A similar experiment to Schwann's was also performed, the air in the apparatus just described being passed over a heated tube instead of through sulphuric acid. Nevertheless, organisms (penicillium and vibriones) developed. Two flasks (instead of three) were used, the first being filled with boiling water (this flask having the three necks as usual, one receiving the heated air, one for the siphon, and the third leading into the second

flask), and the second containing boiled decoction of hay and having two necks. He states that he succeeded more often in obtaining organisms when filtered, unboiled water was substituted for the boiled water; and, as he has already proved, as he thinks, that water does not contain organisms, he considers it a matter of indifference whether the water has been boiled or not!

Similar experiments with *artificial air* yielded the same results.

A new objection is then brought forward, viz., that if the atmosphere be the universal disseminator of germs, the greater the mass of air in contact with a fluid, so much the greater ought to be the number of organisms which grow in that fluid. This he has not found to be the case, and he also points out that in flasks containing the same fluid, placed close to each other at the same time, different forms of organisms may develop, a fact which he does not consider capable of explanation on the theory of Panspermism.

Pouchet further examined *air dust* microscopically, and he admits that he finds spores of fungi and other microscopic organisms, but he does not think that they are ever present in sufficient quantities to explain the numbers which are found in an infusion, for he states that the bacteria themselves do not increase in number by fission (or but very slowly), but that on the contrary each is spontaneously generated.

To prove that organisms do not in the main come from the air he took five grammes of dust from the roof of the Rouen Cathedral, and placed it in 100 grammes of distilled water. Then in another similar vessel he placed 100 grammes of distilled water, containing five grammes of the dry stalks of China aster, previously exposed for two hours to a temperature of 200° C. These two vessels were placed under the same glass shade. Eight days later the first contained vibriones, monads in small numbers, and a few kolpodes. The second was full of monads and kolpodes.

During a later research Pouchet examined the dust which collected in the lungs and bones of birds, and he states that there can always be found evidences of the locality in which the birds lived. For instance, this dust in the case of forest

birds contains fragments of wood, leaves, starch, &c. He says that he never found spores of plants nor ova of microscopic animals nor encysted animalculæ. That he should not have found spores of fungi, which one would think are more numerous in the forest air than starch granules, is inconceivable, unless it is supposed that there is a special provision in the animal economy which prevents their entrance into the lungs, though permitting the admission of starch granules. The probability is that he did not recognise them.

Not only does Pouchet support the doctrine of the spontaneous generation of bacteria and the lowest forms of animal or vegetable life, but he is also prepared to accept the spontaneous origin of fleas, acari, and cysticerci!

If, now, we carefully examine these experiments, we shall see that they are full of the grossest blunders. Take, for instance, his experiments to show that organisms are not present in putrescible substances, in water or in air. The first experiment is simply absurd. Some seeds are charred and introduced into a vessel containing distilled water, and covered with a bell jar; organisms develop. What does this prove? Does it prove that organisms were not present and may not constantly be present in putrescible substances exposed to the air? No. It merely proves that this cannot be their only source; for here the water and the air were not heated or otherwise purified, and therefore the organisms might be easily communicated through them.

Again, to prove that water does not contain them, an artificial water is prepared, boiled for a quarter of an hour, placed in an unpurified vessel containing some hay, which had been previously heated and exposed to ordinary air. What does this show? Certainly not that ordinary water does not contain organisms. Even admitting that this specimen of water did not contain them, there was ample explanation of their presence from the fact that the fluid was put into an impure vessel, and that the air had access to it.

Nor is Pouchet more successful in his attempt to show that the air is not the vehicle.

In considering the question as to the presence of organisms or their spores in the air Pouchet puts forward the idea that if

organisms were present in the atmosphere in sufficient numbers the atmosphere would be totally obscured. But it has been asserted and shown by Pasteur and others (as we shall see later on) that organisms are by no means so numerous in the atmosphere as was formerly supposed, but that they are generally derived from dust which has settled or from water. Further, Tyndall has shown, by means of the beam of light, what numbers of minute particles fill the air around us, and up to a certain point, instead of obscuring it, really render the light visible.

In his repetition of Schulze's experiment it must be admitted that Pouchet has a stronger case, but even here the flask and the tubes were not purified, the quantity of fluid as compared with the size of the flask was very small, and there is always the possibility of a flaw in the cork or in the joinings of the various tubes. And further, this experiment loses its force when Pouchet admits that he does not always get organisms, and states, on the contrary, that, when a simple apparatus is employed, a negative result is obtained. Thus to quote his own words: ‘Dans un appareil à simple rentrée d'air’ (this consists of a flask having only one tube passing through its cork, to which tube Liebig's bulbs are attached, see Fig. 61), ‘et dont les boules de Liebig contenaient de l'eau, on remplit le tiers du ballon de colle de farine légère: que l'on y tient quinze minutes en ébullition à l'aide d'une lampe. Celle-ci éteinte, l'air rentra dans l'appareil en traversant l'eau peu-à-peu. L'appareil fut abandonné deux mois à une température moyenne de 14 degrés, et pas la moindre moisissure ne se déclara à la surface de la colle durant tout ce temps.

‘Au contraire un critérium, placé à côté et en contact avec l'atmosphère, avait au bout de cinq jours toute sa surface envahie par des champignons.

‘Une expérience enterprise le même jour et dans les mêmes conditions, mais dans laquelle l'air est introduit dans l'appareil en traversant des boules de Liebig remplies d'acide sulfurique, donna absolument les mêmes résultats.’

Thus Pouchet showed that not only was Schulze's experiment successful, when performed with a simple apparatus, but he further demonstrated that it was not necessary that the air

should pass through sulphuric acid; if it were merely *washed in water* it was sufficient.

Again, 'Dans notre appareil à simple rentrée d'air, et dont les boules étaient remplies d'eau, on mit 175 grammes d'urine humaine et on l'y tint en ébullition pendant un quart d'heure. Ensuite l'air rentra en traversant l'eau et l'appareil fut abandonné sous l'influence d'une température moyenne de 12 degrés. Deux mois après, l'urine était encore parfaitement limpide et pas la moindre moisissure ne s'était déclarée à sa surface. Un critérium placé à côté, au bout de huit jours, était envahi par une abondante végétation cryptogamique.

'Une expérience est faite le même jour et absolument dans les mêmes circonstances, seulement l'air ne rentre dans l'appareil qu'en traversant des boules remplies d'acide sulfurique. Le résultat est absolument le même que dans l'expérience précédente: l'urine est intacte.'

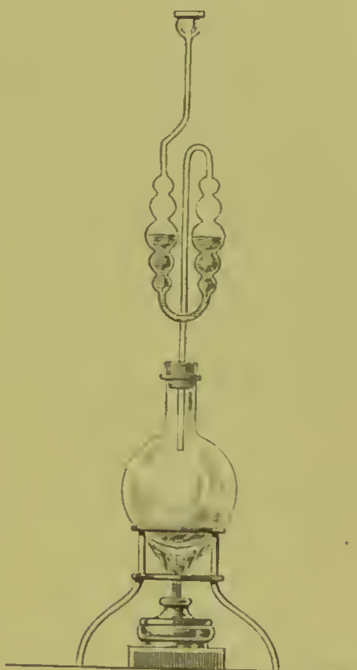


FIG. 61.—A SIMPLE MODE OF REPEATING SCHULZE'S EXPERIMENTS (FROM POUCHET).

Such are examples of Pouchet's general results with this simple apparatus, and, when he states that with the more complicated arrangement, even with much more prolonged boiling, he generally obtains cryptogamic vegetation, I do think that I am only drawing a fair inference when I suppose that there was something defective about his apparatus.

Nor is he more fortunate with Schwann's experiment. Referring to Pasteur's results—that when he (Pasteur) performed Schwann's experiment of having in contact with fermentescible substances only previously heated air, he obtained neither fermentation, nor yeast, nor infusoria—he says: 'L'air calciné a ici encore arrêté la fermentation et les produits organiques qui en dérivent; cet air est donc également impropre au développement de phénomènes chimiques, comme il l'est à celui des phénomènes vitaux. L'expérience de

Schwann, et celles qui ont été calquées sur elle, sont donc absolument insignifiantes.' Truly a strange interpretation of the facts, more especially as he had previously attempted to demonstrate that calcined air and air which had previously passed through sulphuric acid were equally incapable of preventing the development of organisms, and as in the 'more rigorous' conditions under which he performed the experiments he found that organisms always developed!

Looking more closely at these so-called 'more rigorous' conditions, we find that they contain a sufficient number of loopholes to explain the result.

What are we to think of the third experiment to show that the air is not the vehicle of these organisms? An impure vessel is taken, containing dust, and unboiled filtered water is introduced into it along with some heated hay. Is it any matter for surprise that organisms are found here, however pure the air? The dust in the vessel contained plenty of their spores, the various tubes passing into it contained dust and impure air, while the water itself was teeming with organisms or their spores. And similar objections may be urged against the first and most rigorous experiment. Boiling hay infusion is introduced into a vessel containing dust; no attempt is made to destroy the septic properties of this dust or to purify the air and the dust in the tubes leading to and from it. Air is now forced into the infusion, carrying along with it the organisms which Pouchet ought to have known were theoretically present in the air in the last connecting tube, as well as on the walls of that tube. This is the 'rigorous' experiment, which is sufficient 'to overturn the experiments of Schwann and Schulze.' It is needless to pursue the criticism of such methods.

The following experiment is stronger, and he considers it sufficient to upset those of Schwann and Schulze. A flask was introduced to the bottom of a vessel containing a decoction of barley, which had been kept boiling for six hours. The flask being completely filled with this fluid, was brought to the surface and corked, and then the circumference of the cork was surrounded by varnish. On the sixth day a deposit of yeast was seen, and the flask burst on the seventh.

Here an impure vessel, cork, &c., were used, and the heat

was not applied to them for a sufficient length of time. Further, the fluid cannot have been boiling when the vessel was introduced and corked, otherwise, if he had done it efficiently, the operator would have scalded his fingers. It is, moreover, noteworthy that Pouchet only got organisms in one experiment of this kind.

In his 'Micrographie Atmosphérique,' Pouchet falls into the great error of assuming that organisms do not subdivide quickly enough to account for their rapid appearance in infusions. Now it has since been made out by several observers that organisms do divide with sufficient rapidity, and of this I may mention two instances from Mr. Lister's work. In the course of observations on a form of micrococcus, which he terms 'Granuligera,' he found that in little more than an hour and a half they had trebled their numbers, a fact which he made out by observing the subdivision of a little group. And with regard to the *Bacterium lactis*, he found definitely by numeration that it doubled by fission in an hour, so that given one *Bacterium lactis* there would be in twenty-four hours no less than 8,388,608 bacteria; and other forms of organisms develop even more quickly.

Then, again, Pouchet objects that with free access of air, a *greater variety of forms* ought to be found, but it must be remembered that what may nourish one form may not be good for another, and that the products of the growth of the form which in the first instance was most vigorous and most numerous, may interfere seriously with the growth of other varieties; and in the experiment which he mentions it is quite evident that in the vessel to which the dust was added there is little or no nutriment compared with that contained in the stalks of the China aster. And lastly, when he places similar infusions, similarly treated, in flasks of the same size, under the same glass shade, and in similar conditions, and finds that the organisms which appear differ in form in the two flasks, he does not obtain, as he supposes, a proof of spontaneous generation, but the contrary. For on that theory the same infusion, in the same conditions, ought to give rise to the same species of organisms; and the occurrence of different forms can only be explained by supposing that different spores gained access to

the various infusions, a view quite in accordance with the theory of Panspermism.

Appearing shortly after Pouchet's work, and leading to diametrically opposite conclusions, were the researches of M. Pasteur, which have by many been considered as administering the death-blow to the theory of heterogeny.

Pasteur,[1] in his account in the 'Annales des Sciences naturelles,' begins by attempting to demonstrate the existence of spores in the atmosphere, a fact which Pouchet had previously admitted. Such attempts are, however, very unsatisfactory, partly from the difficulty of recognising what are and what are not spores, and also from the fact that if spores do exist they must be so excessively minute as to be in many cases invisible under the microscope. Professor Tyndall has shown, by means of the condensed beam of light, the existence of innumerable solid particles in fluids, in which but few could be detected by the microscope. Pasteur certainly demonstrated, and this is generally admitted, that spores of fungi do occur in the atmosphere. It is not necessary, however, to have this demonstration of the existence of spores, for the matter can be set at rest by experiment alone, and it is these experiments, and not the demonstration of the existence of spores of fungi in the atmosphere, which give the value to Pasteur's work.

Operating with an albuminous saccharine fluid, in the manner described below, Pasteur always succeeded in preventing the growth of organisms in that liquid, in presence of heated air. 'J'ai certainement eu l'occasion de répéter plus de cinquante fois l'expérience, et, dans aucun cas, cette liqueur, si altérable, n'a donné vestige de productions organisées en présence de l'air calciné.'

Into a flask with a capacity of 250 to 300 c.cm. were introduced 100 to 150 c.cm. of the saccharine albuminous fluid. The neck of this flask, which had been drawn out, was then connected with a platinum tube in which the air could be raised to a very high temperature. The fluid was boiled for two or three minutes, and then the calcined air allowed to enter.

Although the experiment succeeded in the case of the

[1] *Annales des Sciences naturelles*, série 4, t. xvi. 1861.

saccharine albuminous material, and some other fluids, it did not succeed with milk. Leaving milk out of consideration for a moment, it was shown that other fluids which, although previously boiled, when exposed to ordinary air, rapidly become the seat of development of organisms, remain barren when exposed to heated air. (It has been remarked by several authors, especially by Pouchet, that Pasteur was unable to succeed, in many cases, in repeating Schwann's experiment with calcined air. This is true: but the experiment which failed was the one where the flask is inverted over mercury, and the calcined air then introduced, and Pasteur has pointed out that it is from the mercury that the source of contamination is derived.) It remained to enquire further what happened when dust which had not been heated was introduced into the

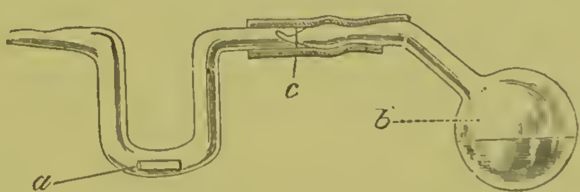


FIG. 62.—PASTEUR'S MODE OF INTRODUCING DUST INTO FLASK CONTAINING CALCINED AIR.

b, the flask containing the infusion sealed after being filled with heated air. *a*, piece of asbestos containing dust. When the tube in which this rests has been purified and filled with heated air, the neck of the flask *b* is broken at *c*, and the dust slipped in. The neck of the flask is again sealed. (From Pasteur.)

fluid in presence of heated air. This was done; the air coming in contact with the boiled fluid was, as in the former case, previously heated, and the fluid had remained barren for one or two months; then precautions being taken that no unheated air was introduced, unheated dust was put into the flask (Fig. 62), and, as a consequence of this, development of organisms rapidly occurred, and these organisms were of the same kind as those which appeared in fluids freely exposed to air. Thus Pouchet's objection to Schwann's experiment (viz., that heated air interfered with the growth of organisms) was completely upset.

Fresh urine behaves in exactly the same manner when treated in the two ways described.

In the case of *milk* Pasteur was, however, unsuccessful when the boiling temperature was maintained for only two or

three minutes (a length of time quite sufficient in the former cases); and this want of success he believes to be due to the fact that in this fluid organisms can resist the boiling temperature for a longer period than in other liquids. Two or three minutes of a temperature of 110° C., or a prolonged temperature of 100° C., were, however, sufficient to sterilise milk. This resisting power he considers to be due to the alkalinity of the fluid, for he finds the same difficulty if he renders his sugared yeast water alkaline by the addition of carbonate of lime. In the case of milk and of the latter fluid, thus purified and preserved unchanged for some weeks, the addition of the atmospheric dust, in the way described, produces the same results as in the fluids previously referred to.

But it is not necessary to use calcined air, ordinary air will be equally inert, if only it has had opportunity to deposit its dust before being admitted into the flask. This he showed to be the case by his well-known experiments with flasks with bent necks. Into a flask a suitable quantity of the fluid to be experimented on is introduced, the neck is then drawn out long and bent in various directions (Pasteur only figures one long curve) so as to present obstructions to the entrance of solid particles along with the air (see Fig. 3, p. 16). The fluid is then boiled for the requisite length of time, and, the lamp being removed, ordinary air is allowed to enter. Fluids may be prepared in this way, and preserved for any length of time desired. On the other hand, if the neck be straight, so as to allow the dust to fall readily into the flask, organisms appear, however long the neck. In the same way, if the neck be broken off short, so that there is no obstruction to the entrance of dust, organisms rapidly develop.

In the case of milk the boiling point must be raised to about 110° by heating the fluid under pressure, or the milk must be boiled for a long time.

That the cause of the growth of these organisms is not continuous, as would be the case were it some known or unknown force, but that it is the advent to the fluids of solid particles floating in the atmosphere, is likewise demonstrated by the following experiment. Flasks containing the saccharine yeast solution were prepared, and sealed during boiling. These

flasks being opened in different places, with certain precautions against the admission of dust from its neck, &c., air rushed in, carrying with it any suspended dust. The neck of the flask was again sealed, so as to prevent any further entrance of dust. It was thus found that the air in some places and under some conditions contained none of the particles which give rise to organisms, while, on the other hand, when they were present the organisms might be of very various kinds. One of the situations in which such flasks could be opened without any development of organisms, was in some cellars which had not been entered for a long time, and in which the dust had therefore settled. Precautions were of course taken that the operator neither introduced the particles himself nor stirred up the dust of the cellar.

I may quote one experiment: Le 14 août 1860 j'ai ouvert et renfermé dans les caves de l'Observatoire dix ballons contenant de l'eau de levûre de bière, et onze autres ballons de la même préparation dans la cour de l'établissement, à 50 centimètres du sol, par un vent léger. Tous ont été rapportés le même jour dans l'étuve de mon laboratoire, dont la température est de 25 à 30 degrés. J'ai conservé jusqu'à ce jour tous ces ballons. Un seul de ceux ouverts dans les caves renferme une production végétale. Les onze ballons ouverts dans la cour ont tous fourni des Infusoires ou des végétaux du genre de ceux que j'ai déjà décrits.'

But the greatest blow was given to the views of the heterogenists when Pasteur demonstrated that albuminoid materials are not necessary for the development of bacteria and fungi, but that they can be replaced by crystalline salts, such as phosphates and salts of ammonia. He prepared a fluid of the following composition, in which these organisms readily grew:—

Eau pure	100	grammes
Sucre candi	10	,,
Tartrate d'ammoniaque	2 à 5	,,
Cendres fondues de levûre de bière . . .	4	,,

'Si l'on sème dans cette liqueur, en présence de l'air calciné, les poussières qui existent en suspension dans l'air,

on y voit naître les Bacteriums, les Vibrions, les Mucédinées &c.'[1]

Here there is no question of albuminoid particles combining to form an organism. If they develop here spontaneously they must be built up from mineral salts.

Such is Pasteur's first work—the work which is considered by many to have struck the final blow at heterogenesis. Let us see what it really does prove.

It shows that a certain number of boiled fluids prone to the development of organisms can be preserved, without any growth of organisms in them, in the presence of calcined air, or of ordinary air, the dust of which has been allowed to settle; that the introduction of dust into these flasks is the only condition requisite for the development of organisms; that the source of organisms is something discontinuous—particulate; that organisms are not necessarily the result of changes in albuminoid materials, for they grow vigorously in an artificial mineral fluid. Further, Pasteur has shown that among the particles present in the dust of the atmosphere there are spores of fungi and bodies which may be bacteria or their spores.

This is all that is *proved* by these experiments; and how does Pouchet answer them? By asserting that these solid particles are not bacteria or their spores, but lifeless particles, which under certain conditions become vivified, and appear as various forms of organisms? The only possible theory, one would think! No. Pouchet disputes the facts. He does not, it is true, take the trouble to repeat Pasteur's striking experiment of the flask with bent neck. He simply says: 'C'était une erreur.' Further, 'Nous avons refusé de répéter les expériences de M. Pasteur parce que, logiquement, rationnellement, pour des physiologistes, du moment où il est reconnu

[1] It may be mentioned here that, since Pasteur published, other similar fluids in which organisms can grow have been employed. Thus Cohn uses the following:—

Distilled water	20 c.cm.
Tartrate of ammonia	·2 grammes
Phosphate of potash	·1 ,,
Crystallised sulphate of magnesia	·1 ,,
Tribasic phosphate of lime	·01 ,,

que celles de Schwann[1] sont absolument erronées, et je pense que pas un seul de ceux-ci ne voudrait aujourd'hui le contester, les expériences du chimiste de Paris sont conséquemment frappées de la même nullité.'[2]

Pouchet again returns to the simple experiment in repetition of Schulze's, which we have previously quoted, but now his statement is directly opposed to that formerly made. He now says, 'En employant dans cet appareil' (à simple rentrée d'air) 'de la colle de farine extrêmement légère, de l'albumine, de l'urine, de la bière, du foin, ou de la noix de galle, *constamment* on voit apparaître des microphytes ou des microzoaires après un temps fort court.' How is this contradiction to be reconciled? Is not the former experiment most likely to be correct?

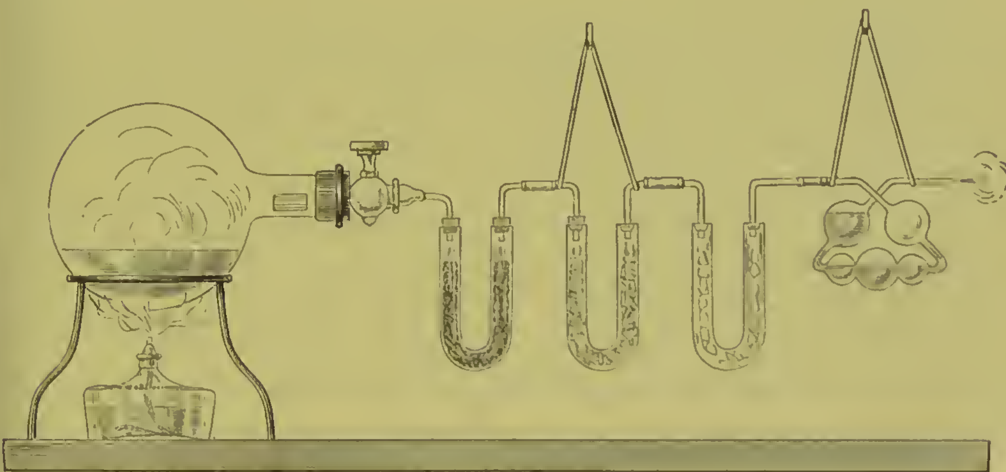


FIG. 63.—ANOTHER MODE OF REPEATING SCHULZE'S EXPERIMENT. (FROM POUCHET.)

He then goes on to describe an experiment 'much more 'rigorous' than that of M. Schulze.

'Dans un ballon dont le col placé horizontalement supporte un robinet, je mets une certaine quantité d'eau ordinaire. Un corps fermentescible renfermé dans un gros tube de verre, et qui a été préa-

[1] I previously pointed out that Schwann's experiment, which was occasionally unsuccessful in Pasteur's hands, was not that to which M. Pouchet alludes, but that performed over mercury; and Pasteur has abundantly demonstrated the fallacy here and how it can be overcome.

[2] See *Nouvelles Expériences sur la Génération spontanée.*

lablement chauffé à 150° C. pendant cinq heures, est placé à l'intérieur du col de ce ballon : ce tube est fermé par un opercule rôdé à l'émeri et scellé hermétiquement avec lui par une substance facilement soluble dans l'eau. Le ballon communique avec trois tubes en U et des boules de Liebig. L'un de ces tubes contient de la ponce sulfurique, un autre de la potasse caustique, et le troisième du coton cardé.

'Les boules de Liebig sont remplies d'acide sulfurique concentré.

'A l'aide d'une lampe on met l'eau du ballon en ébullition pendant dix minutes, et ce n'est qu'ensuite qu'on articule les tubes en U à l'extrémité du tube de Liebig. Enfin on éteint la lampe, et, tandis que l'appareil se refroidit, l'air extérieur y rentre en traversant l'acide sulfurique, le coton, la ponce sulfurique et la potasse. Quand l'appareil est parvenu à la température ambiante, on fait tomber le tube dans le liquide, et lorsque celui-ci a dissous la substance soluble qui lute l'opercule, ce dernier s'ouvre et l'eau envahit l'intérieur de ce tube.

'Peu de temps après, constamment on voit la liqueur du ballon se peupler de végétaux et d'animaux microscopiques, selon les substances que l'on a confinées dans le tube. Jamais en suivant ce procédé l'expérience ne manque.'

Such is the experiment which is more rigorous, certainly much more complicated than Schulze's. Nevertheless there are numerous loopholes in it.

Thus this little tube containing the fermentescible material is cold, and may be covered with dust when introduced into the neck of the flask. It is certainly strange that, in order to be certain that organisms should develop, it is necessary to heat the great bulk of the water used, and the putrescible material in a small quantity of water separately, and then to unite them in the manner described. For Pouchet himself admits that he does not always get organisms with Schwann's simpler method.

Then, again, it is no easy task to cork thoroughly a vessel heated to the boiling point of water; and the cork used was in no way purified. It is true that steam passed over it for a short time, but then steam is dry heat, and it is generally admitted that dry heat at the temperature of boiling water is not sufficient to destroy all organisms. This argument also applies to the dust on the wall of the vessels. And then in tilting the vessel to introduce the tube into the water, the joints, unless very securely fixed, may open, or living dust may be shaken into the

fluid from the neck or cork, leaving out of consideration the fact that it is in all probability carried in with the small tube.

Pouchet looks on this experiment as completely disproving those of Schulze, Schroeder and Dusch, and Pasteur, and why? Because with this apparatus he constantly obtains organisms. How then does he explain Pasteur's results? Pasteur boils his fluids for two or three minutes, and finds that they remain barren. This experiment can only be answered by Pouchet (so long as he adheres to this line of argument) by supposing that Pasteur is making a false statement, or that by boiling his fluid he has destroyed its power of producing organisms spontaneously. As regards the first, a committee composed of the first scientific men in France confirm the truth of what Pasteur asserts; while the second can have no force when looked at in the light of Pouchet's own experiments; for we find that the latter obtains organisms after keeping his fermentescible material at a temperature of 110° C. for several hours, and boiling the water for from 30 to 60 minutes.

Such a method of reasoning and of experimentation would not have deserved so much notice were it not for the great influence which Pouchet's work has exercised and does still exercise on those who have not read or studied it. And I have felt it the more necessary to consider his work in detail as he is one of the last observers who has maintained a more or less complete heterogenesis, and also as I am not aware of any work in which his experiments have been subjected to a searching criticism.[1]

[1] Pouchet's experiments and conclusions were objected to by Milne-Edwards, Payen, Quatrefages, Claude Bernard and Dumas, who examined them, and also those of Pasteur.

CHAPTER IX.

SPONTANEOUS GENERATION (*continued*).

The experiments of Jeffries Wyman: explanation of the results: Wyman's views on the subject. Dr. Bastian's views: Cases in which it is still possible that abiogenesis may occur: Growth in vacuo—Bastian's experiments —my own results—Cohn's facts—Dr. Roberts's objections, the walls of the vessels remain impure—Bastian's reply—Objections to the latter—Gruithuisen's experiments—Paul Bert's results with compressed air—Pouchet looks on a vacuum as preventing spontaneous generation—Paul Bert's results with rarefied air—Dr. Bastian does not always get positive results: Experiments in airless and hermetically sealed flasks raised to a high temperature—Objections—Prof. Huxley's and Dr. Sanderson's statements—Ray Lankester's results—Hartley: experiments with alkaline fluids—Roberts's counter-experiment. Mr. Lister's experiments. Experiments by Roberts and Tyndall.

OTHER writers, chiefly French and Italian, among whom may be mentioned Joly, Musset, and Mantegazza, have supported Pouchet, but as their experiments furnish little or no additional evidence nor new argument I think it unnecessary to discuss them. Those of Joly and Musset will be found in the 'Comptes Rendus de l'Académie des Sciences,' about the same period as the papers of Pasteur and Pouchet.

I must, however, refer at length to the experiments of Professor Jeffries Wyman, of Cambridge, U.S. These have been largely quoted by the supporters of heterogenesis, as proving their view, though it ought to be borne in mind that Wyman himself expressed no such opinion. It must be confessed that at first sight the experiments seem difficult of explanation on the Panspermic theory, and it is the more necessary to scrutinise them carefully, as he has evidently approached the subject with a perfectly unbiassed mind, and has therefore simply recorded his facts without attempting to force any definite conclusion on this question from them.

The following are the facts which have been adduced by supporters of spontaneous generation as favouring their views.

Flasks were prepared in three ways:

1. 'The materials of the infusion were put into a flask' (the general relation between the quantity of fluid and the capacity of the flask was, that about 20 c.cm of fluid were introduced into flasks of about 500 c.cm. of capacity) 'and a cork through which passed a glass tube drawn out to a neck was pushed deeply into the mouth of it. The space above the cork was filled with an adhesive cement composed of resin, wax, and varnish. The glass tube was bent at a right angle and inserted into an iron tube and cemented there with plaster of Paris. The iron tube was filled with wires, leaving only very narrow passage ways between them.'

Into these flasks such fluids as sugar, gelatine, and hay infusion—cheese, sugar, and gelatine—flesh, sugar, and gelatine, &c., were introduced, and boiled for periods varying from fifteen minutes to two hours, while at the same time the iron tube, filled with wires, was heated to redness. On withdrawing the lamp from the flask, the air which entered passed over these heated iron wires. When cold the flasks were sealed with the blow-pipe. Fourteen vessels were prepared in this way, and in ten of these, when opened after the lapse of various periods of time, organisms were found, generally vibriones and bacteria. The other four remained barren.

2. In a second set of experiments the cork in the neck of the flask was avoided, the neck itself being drawn out and bent at right angles, and into the orifice of this tube the iron tube was cemented. The other conditions were the same as in No. 1.

Similar fluids were used here as in the former case, such as gelatine and sugar with a few drops of urine and milk, beef infusion, &c. Thirteen flasks were treated in this way, and in all organisms appeared.

3. In others the flask was sealed at the ordinary temperature of the room, after the fluid to be tested had been introduced, and then it was submerged for a variable period in boiling water. This was a repetition of the experiments of Needham and Spallanzani. In all the flasks so treated organisms developed.

Four experiments were made under pressure, and of these two gave evidence of life ('monads and vibrios'), the other two remaining barren.

Such facts coming from an accurate and totally unprejudiced observer cannot be dismissed lightly. It is quite evident, on reading Wyman's paper, that the facts are accurately narrated, and we must therefore see whether any flaw can be detected in the method of experimentation, and we must attempt to find some explanation of results so diametrically opposed to those obtained by Pasteur which are, it must be remembered, equally indisputable.

Now, if we compare this method with that adopted by Pasteur, we shall see that with one exception the essential details are the same. This exception is, however, an extremely important one, and is probably the explanation of the diverse results obtained by several honest workers, and even by the same worker at different times. Pasteur takes a flask having a capacity of 250 to 300 c.cm., and into this 100 to 150 c.cm. of the liquid are introduced. Wyman uses flasks varying from 500 to 800 c.cm. in capacity, and into these he puts 12 to 40 c.cm. of the liquid. (In neither case was there any attempt at preliminary purification of the walls of the flask or of the air in the interior.) In Pasteur's experiments the fluid occupies $\frac{1}{2}$ or more of the capacity of the vessel; in Wyman's only $\frac{1}{20}$ to $\frac{1}{30}$ part.

Such is the only important difference between their methods; and this affords, I believe, sufficient explanation of the opposite results. For in Pasteur's flask only a proportionally small part of the wall of the flask has to be purified by the steam, and the extent of this part is of course much diminished by the ebullition of the fluid during boiling. There is also in Pasteur's flask only a very small quantity of air, with its dust, to be acted on. It is thus not to be wondered at that a barren result was obtained. But in Wyman's experiments by far the greater part of the flask and of its contents is impure, and can only be purified by the steam. Now steam, as heat, must be looked on as dry heat, and it is stated by Wyman, in a later publication,[1] that certain forms of organisms may

[1] *American Journal of Science*, vol. xliv. 1867.

resist the prolonged application of even a higher dry temperature than 212° F. Wyman also points out that the temperature of the air even half an inch above the surface of boiling water is many degrees below the boiling point. How much lower then will this temperature be at the orifice of this large flask during the greater part of the time in which the fluid is boiled? But even admitting that steam is moist heat (what I am by no means disposed to allow) several remarkable instances of vegetable growth at high temperatures are produced by Wyman, in one case even at a temperature of 208° F.

Such is the explanation I would give of Wyman's results, and that this is a true explanation will be very evident when I come to the consideration of the method of experimentation adopted by Mr. Lister. This explanation accords in every way with my own experience, in which I could point to several similar instances.

As I have said, Wyman is generally quoted in support of the theory of spontaneous generation, and at one time I thought that he had entertained that view, but the following facts brought to my notice by his brother, Dr. Morrill Wyman, show that he never gave any expression of opinion on this point, and that he appreciated the possibility of such an explanation of his results as I have given.

His first article is entitled 'Experiments on the formation of infusoria in boiled solutions of organic matter enclosed in hermetically sealed vessels and supplied with pure air,'[1] and his second, 'Observations and experiments on living organisms in heated water.'[2] With regard to the object of his research he says, 'The observations and experiments contained in this communication have not been brought together either for sustaining or refuting the doctrine (spontaneous generation) just referred to, but partly with the view of testing the accuracy of the experiments formerly made, and chiefly for the purpose of determining how far the life of certain kinds of low organisms is either sustained or destroyed in water which has been raised to a high temperature, a result which must be reached before spontaneous generation can be either asserted or denied.'

[1] *Silliman's Journal*, vol. xxxiv. 1862.
[2] *Ibid*, vol. xliv. 1867.

With regard to his experiments he says, 'In the first experiments the red hot tube, beyond a question, destroys all organisms contained in the air which enters the flask through it, but is without effect on such as may be contained in the solution, or adhere to the inner surface of the glass. These come in contact only with boiling water or steam, and unless destroyed by one or the other of these would be sufficient to vitiate any experiment, however careful the adjustment and heating of the tube may have been. We therefore believe that the tube is an unnecessary and useless complication of the apparatus.'

In another set of experiments it was shown 'That if the boiling of the flasks was continued for four hours, the infusoria may appear nevertheless—though in other cases it has happened that life ceased to be manifested if it was continued only for two hours.' 'In pushing the experiment still further, we have not found that infusoria appeared in any instance if the boiling was prolonged to five or six hours.' Several experiments, in which many flasks were used, were tried, but 'the result was uniformly the same. Thus a limit to the development of infusoria in boiling water was reached.' Dr. Wyman tells me that in the summer of 1880 he examined one of these flasks, which is marked as having been prepared in June, 1867, and which has remained unopened ever since. 'Judging by the signs above given' (absence of scum, of muddiness, or of fermentation) 'there is no evidence of infusorial life.'

The last defence of heterogenesis which it is necessary to consider is that by Dr. Bastian.[1] He gives up the theory of organic molecules *derived from previously living molecules*, and attempts to demonstrate that vital force and living matter may arise *de novo* under the action of the ordinary physical forces—heat, light, electricity, &c. This change of front on the part of the heterogenists is clearly brought about by the overwhelming evidence produced against Pouchet's views, and more especially by Pasteur's success in cultivating organisms from dust in fluids containing no organic matter. A further admission is made which somewhat simplifies the question, viz., that organisms have the power of self-multiplication.

[1] *The Beginnings of Life*, 1872, &c.

The limitation of cases of spontaneous generation, which has been gradually taking place, is exceedingly instructive. Beginning with the higher animals, it became gradually more limited, frogs, flies, &c., being by degrees excluded, till now it is only in the case of the lowest forms of life that the doctrine is asserted, and even there only in certain instances. The cases which are yet doubtful are given by Bastian in the work quoted, and may be grouped into three divisions.

I. The first division relates to the development of organisms in various fluids, more especially *in vacuo*—a condition which Pouchet looked on as inimical to life!

Into flasks portions of various infusions were introduced. The latter were then boiled for from ten to twenty minutes, and hermetically sealed while still boiling. The fluids used were turnip and hay infusions, and also solutions of certain salts, chiefly citrate of iron and ammonia containing portions of wood, cheese, &c.

The conditions of the first experiment mentioned are very striking and unusual.

'A closed flask containing a very strong infusion of hay (boiled for five minutes), *to which had been added $\frac{1}{20}$th part of carbolic acid*, was opened twelve days after it had been hermetically sealed.' Bastian states that this flask contained organisms of a peculiar form.

Such a statement as this, that a saturated solution of carbolic acid (for a watery fluid at the ordinary temperature containing $\frac{1}{20}$th part of carbolic acid is saturated) can *permit* the growth of organisms, is absolutely opposed to all experience and experiment. In experimenting with turnip infusion, cucumber infusion, &c., I have never been able to grow any sort of organism in these fluids, when they contained a larger proportion of carbolic acid than $\frac{1}{275}$th part, even though several drops of fluids swarming with bacteria were introduced. Further, I have lately performed the following experiment:—In January 1880 I introduced carbolic acid into flasks containing strong unboiled hay infusion so as to have a strength of the acid present, varying from 1 in 20 to 1 in 200 parts. These flasks were then covered with cotton-wool caps, and placed in an incubator. When examined six weeks later, there had not yet appeared in any one of them

any sort of organism. And lastly, this statement, that organisms can develop in acid fluids after boiling, is contrary to the whole tenor of Dr. Bastian's later remarks, for his strong point is the development of organisms in alkaline—not in acid—fluids after prolonged boiling.

Bastian also employs turnip and hay infusions (without carbolic acid) and solutions of such salts as citrate of iron and ammonia, and he finds that a slight sediment occurs which contains organisms. He generally has to introduce such things as deal wood, cheese, &c., in order to get this result.

With regard to experiments on such fluids as hay infusion and turnip infusion without cheese, I may state that I have quite lately repeated them with exactly opposite results. At first I proceeded to repeat them, following closely Dr. Bastian's directions, in the expectation of getting organisms, and looking out for some explanation of their occurrence. The physical forces, or whatever else it may be, were, however, not favourably disposed for spontaneous generation at the time and place where I performed those experiments, for to my surprise I was unable to obtain any development of organisms. I tried several modifications, in the hope of finding the cause of their absence, but whichever of these vegetable fluids I used I was able, with proper precautions, to preserve them with the greatest ease. Some specimens were very difficult to filter, and in some a slight muddiness occurred on boiling, and the granular deposit might very readily be mistaken at first sight for organisms, though some care and experience would easily prevent such an error. But I have boiled the fluids for a few minutes and then filtered them under pressure (I could not in this way remove any of Dr. Bastian's supposed physical forces), and having thus obtained a perfectly clear liquid, I treated it like the others. There was now no deposit, and nothing which could be mistaken for organisms.

No doubt other observers have produced evidence which apparently at first sight supported Dr. Bastian's views. I refer to the class of experiments in which prolonged boiling was required for sterilisation, but many of these results depend, I believe, on the same causes as Wyman's, viz., imperfect purification of the walls of the flasks and of the air in their interior,

while the fact, that in some instances such resistance was met with, surely implies the presence of some form of encysted organism or resisting spore, or of an organism placed under conditions in which it is not perfectly heated, rather than some rare form of organic molecule or physical force.

The former view—that there is present in the infusions some form of resisting spore which can withstand the high temperature—was shown by Cohn to be correct in the case of the experiments where portions of cheese were introduced.[1] He repeated Bastian's cheese experiments with great care, and found that after exposure to a temperature of 100° C. for ten minutes, organisms still developed in the mixture of cheese and turnip. He, however, observed that these organisms were always of one form (*Bacillus subtilis*), and that *Bacterium termo* and other forms were absent. On investigating this subject further he found that these bacilli did not merely grow in the form of long rods, but that they produced spores, and he had previously ascertained [2] that the spores of these organisms were possessed of peculiar resisting powers. Indeed, such was their power of endurance under high temperatures, that if some satisfactory explanation could be given why they should always occur in these experiments, the whole mystery would be solved, and the theory of spontaneous generation would no longer be supported by these facts.

Cohn therefore turned his attention to the manufacture of cheese. The Swiss cheese is made in the following manner: milk is placed in large copper vats, and is coagulated by the addition of rennet. This is allowed to stand for a quarter of an hour, and then, after having been kept at a temperature of from 55° to 60° C. for an hour, it is broken up into small masses. These are now taken up in a cloth, placed in a mould, and pressed for twenty-four hours. The cheese is then taken out of the mould, transferred to a cellar, and kept at a temperature of 10° to 12° C. for several months, salt being daily rubbed over its surface. Lastly, it is stored till it attains its full ripeness.

[1] See *Untersuchungen über Bacterien*, *Cohn's Beiträge zur Biologie der Pflanzen*, Erster Band, Drittes Heft, p. 188.

[2] Cohn, *ibid.*, Heft 2, p. 176.

The only stage in the process which it is necessary to consider is the ripening of the cheese. Cohn points out that this is a true fermentation due to the growth of organisms; this

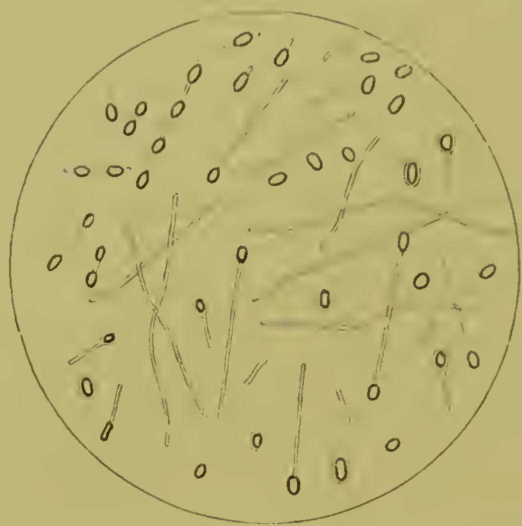


FIG. 64.—BACILLUS SUBTILIS; WITHOUT SPORES; WITH SPORES IN THE RODS; FREE SPORES; × 600 (AFTER COHN).

fermentation begins during the first twenty-four hours, while the curd is still under the press, and is accompanied by the development of large quantities of gas. The slower development of this gas, which occurs later, explains the formation of cavities in the cheese. The chemical change consists in the partial transformation of the milk sugar into butyric acid. The preliminary heating to 55° or 60° C. kills all the organisms except the bacilli which give rise to this butyric fermentation. On examining the rennet Cohn found that it was full of bacilli, many of which contained spores, and of free spores. By the addition of the rennet to the milk enormous numbers of these spores are sown in it, and the subsequent stirring mixes them thoroughly with it. These spores escape death at the temperature of 55°—60° C., and develop in the cheese, thus causing its ripening. They have been shown to resist high

FIG. 65.—DEPOSIT IN RENNET, CONSISTING OF A MASS OF MICROCOCCI AND SPORES OF BACILLI, × 600 (AFTER COHN).

temperatures, and, when used in Bastian's infusions, they are not destroyed, and thus we have a satisfactory explanation of the frequent development of bacilli in these experiments.[1]

The other view—that the organisms were imperfectly heated—was urged against Dr. Bastian's experiments by Dr. Roberts as long ago as 1873. After pointing out that atmospheric germs may get into the flasks at the time of sealing, he goes on to a second source of error, which he considers much more important. 'It is this: Dr. Bastian's process does not insure that the entire contents of the flask are effectively exposed to the boiling heat.' He refers to the difficulty in boiling milk and other substances, owing to the spurting and frothing of the fluid; but he shows that if this is avoided by simply *immersing the flasks* in boiling water, the difficulty in rendering them barren is overcome. He says: 'The essential conditions of the experiment are first the effective exposure of the whole contents of the flask to a boiling heat; secondly, the absolute prevention of any fresh entrance of extraneous solid or liquid particles; and the conclusion I have come to is that if these conditions are rigidly observed, the flasks remain barren. If they do not remain barren it is simply because one or other of these conditions has not been observed.'

In answer to this Dr. Bastian[2] replies: 'I feel quite sure that in my experiments no portion of the inner surface of the glass has escaped the scathing action of the boiling fluid. The vessel has generally been more than three-fourths full before the process of heating has been commenced, so that where ebullition occurs the fluid has always swept over the previously uncovered inner surface and, as Dr. Sanderson testifies, "during the boiling some of the liquid was frequently ejected from the almost capillary orifice of the retort." The inner surface of the vessel was, in fact, always thoroughly and repeatedly washed with the boiling fluid, nearly half of which has been spurted away in order that I might effect this object.'

Now it is just the spurting of the liquid which is so danger-

[1] The experiments of Huizinga, on which Bastian lays great stress, have been refuted by Samuelson (*Pflüger's Archiv*, viii. p. 277) and by Gscheidlen (*Ibid.* ix. p. 163).

[2] *Nature*, February 27, 1873.

ous, for, with the bubbles, solid particles are carried up and deposited on the neck or sides of the flask, out of reach of the boiling liquid, and they may not be acted on by the frothing fluid. I also very much doubt if a bubble of steam sweeping over the wall is to be regarded as a very efficient way of applying moist heat; certainly it is not so efficient as boiling in a fluid. That greater success is obtained when this spurting and frothing do not take place has been stated by Dr. Roberts, and this statement is quite confirmed by Mr. Lister's experience with milk, where he uses the method of immersion with perfect success, and for the same reasons.

But surely this view, that the walls of the vessels remain impure, is the only way in which Bastian's facts can be reconciled with Gruithuisen's experiments mentioned by Bastian himself in a paper read before the Royal Society on March 20th, 1873. I will just quote Dr. Bastian's remarks and experiments in connection with this paper. It is to be observed that Bastian used this method for ascertaining the death-point of bacteria, and the title of the paper in which these statements occur is, 'On the temperature at which bacteria, vibriones, and their supposed germs are killed when immersed in fluids or exposed to heat in a moist state.'

He says: 'It was pointed out by Gruithuisen early in the present century, that many infusions, otherwise very productive, ceased to be so when they were poured into a glass vessel whilst boiling, and when this was filled, so that the tightly fitting stopper touched the fluid. Having myself proved the truth of this assertion for hay infusion, it seemed likely that, by having recourse to a method of this kind I should be able to lower the virtues of boiled hay and turnip infusions to the level of those possessed by the boiled saline solution with which I had previously experimented, that is, to reduce them to a state in which, whilst they appear quite unable of themselves to engender bacteria or vibriones, they continue well capable of favouring the rapid multiplication of such organisms.

'This was found to be the case, and I have accordingly performed upwards of 100 experiments with inoculated portions of these two infusions raised to different temperatures. The mode in which the experiments were conducted was as follows:

'Infusions of hay and turnip of slightly different strengths were employed. These infusions having been first loosely strained through muslin, were boiled for about ten or fifteen minutes, and then whilst boiling strained through ordinary Swedish filtering paper into a glass beaker, which had previously been well rinsed with boiling water. A number of glass bottles or tubes were also prepared, which, together with their stoppers or corks, had been boiled in ordinary tap water for a few minutes. They were taken out full of the boiling fluid, and the stoppers or corks being at once inserted, the vessels and their contents were set aside to cool. When the filtered infusion of hay or turnip had been rapidly cooled down to about 110° F. (by letting the beaker containing it stand in a large basin of cold water), it was inoculated with some of a turbid infusion of hay swarming with active bacteria and vibriones, in the proportion of one drop of the turbid fluid to each fluid ounce of the now clear filtered infusion. The beaker was then placed upon a sand bath, and its contained fluid (in which a thermometer was immersed) gradually raised to the required temperature. The fluid was maintained at the same temperature for five minutes by alternately raising the beaker from and replacing it upon the sand bath. The bottles to be used were then one by one uncorked, emptied, and refilled to the brim with the heated inoculated fluid. The corks or stoppers were at once very tightly pressed down, so as to leave no air between them and the surface of the fluids. The beaker was then replaced upon the sand bath and the gas turned on more fully, in order that the experimental fluid might be rapidly raised to a temperature 9° F. (5° C.) higher than it had been before. After five minutes' exposure to this temperature, other bottles were filled in the same manner, and so on for the various temperatures, the influence of which it was desired to test.'

These bottles were kept at a temperature of from 65° to 75° F. The results were as follows:—

'The experimental results here tabulated seem naturally divisible into three groups. Thus, when heated only to 131° F. all the infusions became turbid within two days, just as the inoculated saline solutions had done. Heated to 158° F. all the inoculated organic infusions remained clear, as had been the case with the saline solutions in my previous experiments, when heated to 140° F. There remains therefore an intermediate heat zone (ranging from a little below 140° F. to a little below 158° F.), after an exposure to which the inoculated organic infusions are apt to become more slowly turbid, although

inoculated saline solutions raised to the same temperature invariably remain unaltered. . . . we may quite safely conclude that bacteria, vibriones, and their supposed germs are either actually killed or else completely deprived of their powers of multiplication after a brief exposure to the temperature of 158° F. (70° C).'

With the conclusions drawn from these experiments as to the death point of bacteria, I, for the most part, agree, but I shall have to refer to them again presently. I have introduced the facts here, because I believe that they add strong confirmatory evidence of one of the explanations of Bastian's results which I have been trying to establish, viz., that in many cases the organisms which appeared in his fluids after boiling did not arise *de novo*, but were derived from particles on the walls and in the air of the vessel, which had not been deprived of life. For in Gruithuisen's experiments and in Bastian's repetition of them, there was no part of the wall of the vessel nor any air in the interior left to be acted on by more or less dry heat. The vessel was filled with fluid, and all the particles in it were subjected to moist heat. And here the high temperatures required in the other cases were not necessary. A temperature of 158° F. continued for a very short time was sufficient to render the liquid permanently barren.[1]

[1] It is of great interest to mention here the difficulties experienced by Dr. Paul Bert in attempting to preserve meat after subjecting it to high pressure ('La pression barométrique,' p. 880).

'Ainsi, dans mes premières expériences, lorsque je voulais conserver une substance, après l'avoir soumise à la compression, je fermais d'un bon bouchon de liége le flacon où elle était placée : ce bouchon était percé d'un trou, et lorsque j'avais retiré le flacon de l'appareil, j'appliquais sur cet orifice fin une goutte de cire fondue, avec laquelle, du reste, je cachetais tout le bouchon.

'Je ne tardai pas à apprendre que cette précaution était insuffisante. Les bouchons, même neufs, même lavés, même chauffés recèlent trop souvent des germes encore en activité. J'eus alors recours aux matras, ballons, tubes, que j'étirais à la lampe, aprés y avoir introduit la substance en expérience ; le trou presque capillaire de la partie étirée permettait à l'équilibre de pression de s'établir.

'Je m'aperçus encore, à mes dépens, que les germes restés à l'état sec sur les parois du petit récipient suffisaient, surtout quand il s'agissait de la putréfaction, mon laboratoire de dissection en étant bourré, pour troubler les phénomènes. Je ne pouvais me mettre sûrement à l'abri qu'en ajoutant un peu d'eau et en remuant avec soin le récipient, avant de la soumettre à la compression, afin de tuer en même temps et les germes contenus dans la substance, et ceux des parois qui se trouvaient mouillés.'

But was the fluid in these vessels in any special condition which prevented the origin of organisms? It is to be observed that when the heat was not high enough to kill organisms, they developed readily, there was then nothing in the conditions which prevented the development of organisms. The only differcnee in the two sets of experiments, and it seems to be indicated by Bastian, appears to be that in Bastian's former experiments the fluids were under diminished atmospheric pressure, while here they were not. The conclusion then apparently is that a vacuum is better suited for the spontaneous origin of organisms than the normal pressure; and that such is Bastian's view is openly stated by him, and among facts in support of it we find the admission that turnip infusion, urine, and sometimes hay infusion, may remain for an indefinite time in Pasteur's flasks with open bent necks without any development of organisms in them, while in a vacuum organisms arise in similar fluids, especially if a piece of cheese has been added to them.

We must therefore see if any other facts favour this *in vacuo* idea. As I have already stated, Pouchet, who is largely quoted by Bastian, states distinctly that a vacuum is most unfavourable for the occurrence of spontaneous generation, and he employs a vacuum for repeating some of Pasteur's experiments, in order to show that as soon as air is admitted, spontaneous generation occurs.

And in regarding a vacuum as inimical to life when compared with the ordinary atmospheric pressure, Pouchet was correct, as will be evident from the following quotations from Paul Bert's recent remarkable work, entitled 'La Pression barométrique.'

On submitting seeds to low pressures he found that *germination* was much delayed.

Thus in his 350th experiment he sowed barley in earth in three pots, and placed them:

A under a glass at the normal pressure.
B ,, ,, at 50c· of pressure.
C ,, ,, at 25c· ,, ,,

Five days later in A the shoots were numerous, very green and very firm, measuring about 10c·

B less numerous, less green, measuring about 8c·

C still less, measure about 6c·

Next day these shoots were cut off at the level of the grain, dried and weighed :

End shoots of A weighed $8^{mg}\cdot8$.
" " " B " $7^{mg}\cdot1$.
" " " C " $6^{mg}\cdot2$.

A low pressure was also found to be inimical to *vegetation.*

Thus to take his 359th experiment :

A number of sensitive plants about $10^{c\cdot}$ in height were each placed under a bell jar on August 1.

August 1. A at $60^{c\cdot}$ of pressure.
B " $50^{c\cdot}$ " "
C " $25^{c\cdot}$ " "

August 3. Some leaves have fallen from C.

August 6. A leaflets sensitive and open.
B " half open and little sensitive.
C " completely closed.

August 7. All restored to normal pressure.

They are all sensitive, but C much less so than the others. C does not close well this evening.

August 9. A is all right, very sensitive.
B. Little sensitive—sickly, yellowish.
C. Leaves falling off—dying.

A similar result was obtained when putrescible substances were submitted to varying degrees of low pressure.

Thus in experiment 386 the muscles of a dog were placed :

A at the normal pressure.
B at $38^{c\cdot}$ of pressure.

Four days later A was horribly putrid.
B is a little less putrid.

Experiment 392.—On January 17, equal sized morsels of meat were placed :

A in a pressure of half an atmosphere.
B at the ordinary pressure.

Other two, C and D containing increased amounts of oxygen.

January 25. The meat, which is the least altered in appearance, is A. The pieces which are most altered are C and D.

Without multiplying the experiments, I may give his results. To quote his own words:

'Si nous envisageons d'abord celles de nos expériences qui ont porté sur la diminution de pression, nous voyons d'une manière nette que dans l'air raréfié la putréfaction a été notablement ralentie et l'oxydation diminuée.

'Mais ces résultats n'ont rien de bien extraordinaire; l'on savait depuis longtemps que la putréfaction *n'a pas lieu dans le vide*, et il était tout naturel de penser qu'elle serait d'autant moins active que l'air serait plus raréfié.'

Dr. Bastian does not always obtain these results (growth of organisms) with infusions containing cheese, &c.[1] If he uses the *rind* of the turnip in preparing the turnip infusion spontaneous generation may not occur! At least such is Dr. Bastian's explanation of his failure to get organisms in one or two instances. An experiment is also narrated in Dr. Sanderson's letter in which the walls of the flasks were thoroughly purified by heat before the introduction of the fluid, in order to see whether the organisms were or were not attached to the walls of the vessels. But this experiment is completely nullified by the mode in which the flasks were afterwards filled, for Dr. Bastian charged them by 'breaking off their points' (they had been sealed when hot) 'under the surface of a neutral infusion of turnips and cheese, freshly prepared for the purpose without employing any of the rind.' Here the previous purification of the walls of the vessels was useless, for they were again soiled by the unpurified fluid passing into the flask.

II. The second series of facts on which Dr. Bastian bases his arguments is, that certain solutions may be exposed in airless and hermetically sealed flasks to a temperature of 270° to 275° F. for 20 minutes, and yet that organisms may subsequently develop in these flasks. Such fluids are chiefly strong infusions of turnip rendered alkaline by liquor potassæ.

Now I have already referred at length to the error that the organisms may not be subjected to moist heat at all, and my remarks apply here also. For 275° F. is not always sufficient as dry heat.

But I would remark—and this may apply to some of the

[1] See letter by Dr. Burdon-Sanderson in *Nature* of January 9, 1873.

first series of experiments—that in only one or two cases were numerous and distinct bacteria found; and I have ventured to think that in some of the other cases the deposit which occurred was simply due to imperfect filtration, and contained the forms described, these forms not having developed since the introduction of the fluid into the flask. For, I would ask, if they had developed anew, why was the fluid not full of them? Why was there only a slight deposit? When organisms are really present in cultivating fluids (as in some of Bastian's experiments) they fill the fluid and render it turbid, often with a scum on the surface.

In some flasks various forms of organisms were found, and fungi were present in the deposit at the bottom, more especially when tartrate of ammonia was used. In some cases Dr. Bastian mixed deal wood with the fluid, and found bodies like vegetable cells, which were undoubtedly portions of the wood. With regard to the fungi, Dr. Bastian has himself pointed out that crystals of tartrate of ammonia, when old, generally contain fungi in their interior.

Professor Huxley, in 'Nature' for October 13, 1871, stated that he had seen Dr. Bastian's experiments and preparations, and expressed his belief that the organisms which Dr. Bastian got out of his tubes were exactly those which he put into them, that in fact he had used impure materials, and that what he imagined to be the gradual development of life and organisation was the simple result of the settling of these solid impurities. For instance, he relates how on one occasion Dr. Bastian showed him a specimen of a fungus developed spontaneously, which Professor Huxley recognised as a fragment of the leaf of a Sphagnum, and that it was so he ultimately, after great difficulty, convinced Dr. Bastian.

On the other hand, it is but fair to Dr. Bastian to admit that these settled impurities were not the only things which he got, and that in reality in some cases undoubted organisms developed. Dr. Burdon-Sanderson, in the letter just quoted, says: 'The accuracy of Dr. Bastian's statements of fact, with reference to the particular experiments now under consideration, has been publicly questioned. I myself doubted it, and expressed my doubts if not publicly, at least in conversation. I

am content to have established, at all events to my own satisfaction, that by following Dr. Bastian's directions infusions *can*[1] be prepared which are not deprived, by an ebullition of from five to ten minutes, of the faculty of undergoing those chemical changes which are characterised by the presence of swarms of bacteria, and that the development of these organisms can proceed with the greatest activity in hermetically sealed flasks, from which almost the whole of the air has been expelled by boiling.' Cheese was used in most of the experiments which Dr. Sanderson witnessed.

Among others who have been unsuccessful in repeating Dr. Bastian's experiments may be mentioned Dr. E. Ray Lankester. In 'Nature' for January 30, 1870, he says, 'In numerous experiments with turnip solution made by Dr. Poole and myself recently in the Laboratory of the Regius Professor of Medicine of this University, we found that under the conditions given in Dr. Bastian's book, no life was developed, a result contrary to that obtained by him in 999 cases out of 1000.'

The fallacy of Dr. Bastian's experiments with saline solutions was well demonstrated as long ago as 1872 by Mr. Hartley.[2] In no instance was he able to confirm Dr. Bastian's statements. In his first experiment he made a fluid consisting of a 5 per cent. solution of tartrate of ammonia and phosphate of soda in distilled water slightly acidified with tartaric acid. Several tubes were filled with these solutions, and were heated for four hours to a temperature of 150° C. They were afterwards kept at a temperature of about 25° C. In none of them did any organisms develop, but in some he found that a slight deposit occurred which apparently was what Bastian had taken for a development of organisms. On examination this deposit was found to be inorganic, and to consist of silica alone. 'The disodic phosphate had attacked the glass, the silica deposited on standing, and hence the jelly-like mass.' He adds further, in reference to Dr. Bastian's use of magenta as a test for fungi, that magenta also stains silica.[3] Hartley does not consider these solutions

[1] The italics are my own.

[2] *Proceedings of the Royal Society*, vol. xx.

[3] Dr. Frankland (*Nature*, January 19, 1871), in whose laboratory Dr. Bastian had performed these experiments, was not satisfied with the results, and repeated some of the experiments, using a solution of carbonate of ammonia

capable of supporting life. In another set of experiments he kept the tubes at a fluctuating temperature, which is another of the conditions which Dr. Bastian considers favourable to spontaneous generation; but here also there was no development. Similar experiments, which gave similar results, were made with turnip infusions and with urine boiled and filtered from mucus. After keeping such fluids *in vacuo* for a long time they were exposed to air, filtered through cotton wool, and kept at fluctuating temperatures without any development; but when they were exposed to unfiltered air under the same conditions, organisms rapidly developed.

Dr. Bastian [1] says: 'The disruptive agency of heat is fairly enough supposed by the evolutionists to destroy some of the more mobile combinations in each solution—to break up more or less completely, in fact, those very complex organic products whose molecular instability is looked upon as one of the conditions essential to the evolutional changes which are supposed to take place.' With regard to this Hartley remarks, 'Before granting such a supposition it would be necessary to know, first, what are the "very complex organic products" of such peculiar "molecular instability" existing in a solution of tartrate of ammonia, sodic phosphate, acetate of ammonia, oxalate of ammonia, in a solution of sugar and calcined yeast, in turnip infusion, or any other putrescible liquid. My experiments show that there is no such disruptive agency in a high temperature; that it does not influence the "more mobile combinations" either in solutions of organic salts or vegetable infusions; . . . Dr. Bastian records the development of organisms in a liquid heated as high as 153° C.; yet the assumed "disruptive agency of heat" is supposed to have influenced the results of Schwann and Pasteur at a temperature of 100° C.! His experience is contradictory to his own theory, and at the same time to the experiments of others to which his theory raises objection.'

and phosphate of soda, as had been done by Bastian in one of his experiments. He also states that the figure of eight particles and bodies which Dr. Bastian had mistaken for living organisms were merely 'particles of glass which had become detached from the inner walls of the tube by the corrosive action of the enclosed liquid at the high temperature to which it had been exposed in the 'digester.'

[1] *Nature*, vol. i. p. 176.

III. The third, and indeed the only series of experiments which can still be held to be worthy of consideration, are those with alkaline fluids which, as is well known, are more difficult to sterilise than acid or neutral fluids. Dr. Bastian states that even though superheated, organisms may develop in them.

This difficulty in purifying alkaline fluids was long ago recognised by M. Pasteur, and was attributed by him to imperfect wetting of the organisms.

However that be, Dr. Roberts [1] has conclusively proved that this is not a case of spontaneous generation, for he has shown that while on the one hand an alkaline fluid is very difficult to sterilise, yet as the same fluid without the caustic potash is very easily rendered barren, and as the caustic potash is pure, if each be sterilised separately and then brought together, without any fresh access of dust, the fluid still remains pure; in other words, the caustic potash does not *determine spontaneous generation*. He shows, in fact, that the potash acts by increasing the resisting power to heat of the particles, which are the forerunners of organisms—not by increasing the abiogenic aptitude of the infusion.

Ten flasks were charged with unneutralised hay infusion. Five of these were simply plugged with cotton wool, and boiled over the flame of a lamp for five minutes. The other five were also plugged with cotton wool, but through the centre of each plug there passed an hermetically sealed glass tube bent obliquely, and containing the quantity of liquor potassæ requisite to neutralise the fluid in the flask. These tubes had been previously heated (after being charged with liquor potassæ and sealed) in oil up to 121° C. in order to destroy any organisms they might contain. The flasks thus prepared were then boiled over the flame for five minutes. At the end of a fortnight their contents were unchanged. The tube was now broken and the liquor potassæ mixed with the fluid. Not one flask germinated; at the end of two months they were still barren. But although these flasks had not acquired the power to germinate, they had acquired *the property of enabling freshly introduced germs to survive a boiling heat*, for when the flasks were unplugged and infected with ordinary air or water and then replugged and boiled five minutes, their contents in every instance germinated in a few days.

[1] *Phil. Trans.* 1874.

I can quite confirm Dr. Roberts's statements, for I have used his method of boiling these fluids separately as an easy mode of obtaining any required degree of alkalinity, and I have never got any results which in the least support the view that the addition of liquor potassæ to any sterilised infusion will make organisms develop in that fluid.

I have already mentioned Mr. Lister's method of procedure in preserving fluids. I have mentioned how successful this was, and how all the difficulties as to tall and small flasks, &c., were overcome simply *by purifying the walls and the air in the flasks*; *by taking care, in the introduction of the impure fluids, to avoid contact with the neck and walls of the flask so purified* (above the level of the liquid); *and by avoiding spurting or frothing during the heating of the fluid.* By Mr. Lister's method all sorts of fluids may be preserved and transferred from one vessel to another, without the development of any organism, with the same certainty as without the occurrence of any fermentative change (see Chapter I.) Mr. Lister's method has removed a great source of error in all these experiments, and I am confident that if his instructions be strictly followed out, the instances of difficulty in purifying fluids will become fewer and fewer. During four years more or less constant work at such experiments, I have only once met with an instance of difficulty in purifying fluids. This case will be alluded to presently.

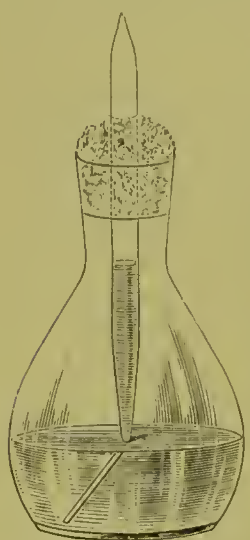


FIG. 66.—DR. ROBERTS'S EXPERIMENT WITH ALKALINE FLUIDS.

The experiments of Dr. Roberts and of Prof. Tyndall [1] as to the absence of fermentative changes in preserved fluids show also the absence of organisms under the circumstances referred to. For their experiments were made with a view to the determination of the question of spontaneous generation. And in my own experience, in order to test various materials, as

[1] For later experiments than those quoted at p. 24, *et seq.*, see Tyndall's paper in the *Philosophical Transactions*, vol. 167, 1877, where some difficulties which he experienced are explained.

to whether they contained organisms or not, I have prepared many hundred flasks of cucumber and turnip infusion, and also many of milk, meat, &c., without in any instance obtaining the slightest evidence in favour of abiogenesis. (The case in which I found difficulty in preserving milk has been already mentioned, and will be alluded to presently.) The rapid souring of milk during a thunderstorm is looked on as a change due to electricity. Accompanying this rapid souring there is a rapid increase of bacteria. I have kept flasks of pure milk for a year, through several violent thunderstorms, without any change taking place in it, and without the appearance of any organisms; and at the end of the year the milk was quite fluid and of normal character, though in a few days after the flasks were opened (they were covered with cotton wool caps) it had coagulated, become putrid, and contained numerous bacteria.

CHAPTER X.

SPONTANEOUS GENERATION (*concluded*).

Facts with regard to unboiled fluids and tissues: Mr. Lister's facts with regard to milk: my own experiments with animal tissues. Present state of the question—Dr. Bastian's latest standpoint: Liebig's doctrine. Can organic molecules derived otherwise than from pre-existing bacteria give rise to bacteria? Firstly, there is nothing unreasonable in looking on these particles as bacteria or their germs: seeds carried by air: excessive minuteness of the germs of bacteria: Mr. Lister's experience with micrococci. Secondly, there is no direct evidence that organic molecules not derived from bacteria can give rise to bacteria: facts with regard to unboiled fluids and tissues—conclusions from these facts: Paul Bert's experiments: Cazeneuve and Livon: Pasteur: Dr. Bastian Thirdly, there is no evidence that active organic molecules (chemical ferments, &c.) can resist heat more than living things can: effects of heat on ptyalin, pepsin, &c.: my own case of difficulty in sterilising milk: Wyman's fact as to an alga living at 208° F.

WITH the great mass of evidence existing against the doctrine of spontaneous generation, I should have no hesitation in casting it aside. But much stronger evidence remains to be alluded to; viz., the behaviour of unboiled fluids and tissues when preserved with certain precautions.

I have already referred to these experiments in a former part of this work, and I may now state that where I have said that fermentation was absent, I might equally well have said that organisms were absent. I have mentioned that blood, milk, and urine could be preserved, unboiled and unacted on by chemical reagents, for an indefinite period, without undergoing fermentation, and, I may now add, without the development of organisms.

Of these I may revert for one moment to Mr. Lister's experiments with milk. Mr. Lister, in his attempts to obtain pure unboiled milk, found that in none of his flasks did he obtain lactic fermentation, but that in all but two (out of

some 50 or 60 flasks) organisms of some form or other occurred, these organisms being in many flasks of totally distinct kinds.

Now these results prove a great deal against the view of spontaneous generation. Two flasks had no organisms at all, therefore milk does not spontaneously generate organisms, does not naturally contain them, has no inherent tendency to undergo fermentation. Then milk which has never been in the dairy does not undergo lactic fermentation; hence the cause of this fermentation is something coming from the dairy, not some form of organic molecule present in the milk. Then not only the fact that in two flasks no organisms developed, but also the great variety which appeared in the different flasks prove that they could not have developed *de novo*. For if we have twelve flasks under the same shade, each containing the same specimen of milk taken at the same time, under the same conditions, any change occurring in one, due to something inherent in milk, or due to some physical force or combination of physical forces acting on it, would have occurred equally in all. But when we see one specimen remaining without organisms, another having a pigment micrococcus, another having bacteria, another fungi, and so on, we must conclude that the appearance of these forms cannot be due to anything inherent in this milk, but that it is due to something which has entered the milk from without.

My own experiments on milk, referred to before, bear out Mr. Lister's statements in every particular, and bring additional evidence, in that they show that a much larger proportion of flasks may be preserved if attempts be made to render the particles in the air, or at least on the udder of the cow and the hands of the milkmaid, inert by the use of carbolic acid or other antiseptic.

Cazeneuve and Livon's experiments on urine have been already referred to, and will be further discussed under the head of fermentation, when they will be found to furnish much additional evidence against the view of spontaneous generation.

After referring to blood, milk, and urine, I also mentioned the experiments on grape juice by Van der Broeck, Pasteur, &c., and on vegetable tissues by Roberts, and I then passed on to

my own experiments on animal tissues. I described the method I had employed, how the tissues were introduced into and preserved in flasks containing cucumber fluid, and how they remained unchanged, and, I may now add, without any development of organisms. That no organisms developed I further ascertained by testing the beakers. By transferring, by means of a pure syringe, a drop of their contents into a flask containing pure cucumber or meat infusion, I found that no organisms developed in the latter, proving that none were present in the former. That organisms would have been found by this method, if they had been present in the fluid, is shown by the fact that, if bacteria were injected into the animal immediately before death, they were found in the beakers and in the flasks. I have also stained the granular matter present in these beakers with methyl violet, according to Koch's method, and have failed to find any organisms (see Plate V. fig. 34).

From all these facts it may therefore be regarded as certain that organisms do not originate spontaneously, in the sense formerly held, under any circumstances, but that they appear in animal or vegetable fluids and tissues as the result of the entrance of *solid particles* into these after their exit from the living organism. That the question has narrowed itself to this, that it is now no longer a question of spontaneous origin of organisms in fluids which have been severely boiled, but that it is a question as to whether *some of the particles* which get into fluids and which resist heat may not be organic molecules not necessarily derived from pre-existing bacteria, or whether they are bacteria or their germs, is evident from what has gone before, and at last Dr. Bastian has formulated similar views in his paper in the 'Nineteenth Century' for February 1878. He says:—

'The modern opponents and supporters of the doctrine of spontaneous generation have always been principally concerned with two sets of problems, (1) As to the nature of the material in the air, the access of which is so apt to induce fermentation in suitable fluids; (2) As to whether some degree of heat below 212° F. can be proved to be always sufficient to destroy

the life of different kinds of living matter in the moist state, but especially that of bacteria and fungus germs.

'In regard to the first set of problems it has been generally agreed for some time that the air contains some germs of living organisms, but that what proportion these bear to the much more bulky, and probably more numerous, organic particles and fragments resulting from the breaking up of previous living matter of various kinds, is uncertain. It has also been generally admitted that any living organisms or germs which chanced to fall from the air into suitable fluids would initiate fermentation or putrefaction therein. The question really requiring to be solved has always been (though it has not been uniformly recognised) whether mere organic *débris* from the air, either in the form of particles or of larger fragments, could or could not also bring about such changes in suitable fluids.'

This view corresponds very closely with the position which Liebig was ultimately driven to take up on the subject of fermentation, and Dr. Bastian says: 'It is Liebig's doctrine which legitimately suggests the doubt above mentioned in regard to the possible potencies of atmospheric particles other than actual germs . . . . If it has been shown that the appearance and increase of the lowest living particles are always a correlative of these processes (fermentation and putrefaction), Liebig's view, if it be true at all, *must be true for the whole of the processes*[1] which are essentially included under the term fermentation.' It will thus be seen that Liebig's theory and Bastian's recent views stand or fall together, and that the facts against one tell equally against the other. Just now I shall only deal with Bastian's views, but their intimate relation with Liebig's theory must be borne in mind, so that the full significance of the facts opposing the latter may be recognised in their bearings also on the former.

Dr. Bastian later on says: 'I go no further than to say that in the present state of the evidence bearing upon the subject I regard the hypothesis of spontaneous generation as the most logical and consistent interpretation of the facts which are at present known.' We must therefore consider what the evidence on this subject is, and whether there are any grounds for accept-

[1] The italics are mine.

ing the theory that the organic molecules which give rise to bacteria originate from matter other than pre-existing bacteria, rather than the view that they are directly derived from pre-existing bacteria, in other words, that these particles are nothing more or less than bacteria or their germs.

1. Firstly, *there is nothing unreasonable in looking on these particles as bacteria or their spores*; indeed, Dr. Bastian admits that some of them are derived from bacteria. But if *some*, why not *all*? Because some resist heat more than others? We shall return to this presently.

We know that the seeds of plants are often wafted by the air, and this is more likely to be the case the lighter the seed, and therefore it is generally admitted that the seeds of fungi, large or small, are carried by the air. Indeed, both Pouchet and Pasteur found and recognised the spores of fungi in air dust.[1]

Further, bacteria propagate by fission in fluids, that is to say, one bacterium develops from another. This process has been often observed. Then, again, some forms of bacteria have been found to produce spores, notably forms of bacilli. Thus the spores of *Bacillus anthracis* have been fully described by Koch, and still more recently the occurrence of small sporules of excessive minuteness has been described by Ewart.[2] Ewart has also asserted that other forms of bacteria, especially of Spirillum, produce spores in like manner. If then in fluids bacteria always grow from bacteria, if some bacteria like fungi produce spores, why refuse to believe that the particles in the air which give rise to bacteria are these organisms themselves or their spores, just as the particles which produce the larger fungi have been shown to be their spores? The excessive minuteness of the bacillus spores renders them difficult of discernment in fluids under the microscope. When dried and shrunk we

[1] In December 1880, having collected a quantity of laboratory dust by aspiration of the air through gun cotton for several weeks, I dissolved the gun cotton in ether and alcohol, and collected the dust on a glass slide: this dust was stained with methyl violet according to Koch's method (see Chapter XII.), and a drawing of it is given in Fig. 35, Plate V. It will be seen that one or two bodies are present which are indistinguishable from bacteria and micrococci, and these bodies have been stained by the methyl violet.

[2] See *Proceedings of the Royal Society*, 1878.

should expect them to be hardly visible; and if the spores of such large organisms as *Bacillus anthracis* are hardly visible, how much less likely are we to find the spores of organisms which themselves can be seen only with difficulty? It seems to me that it would be a very extraordinary thing if in the case of these *minute organisms alone*, the particles which gave rise to them were not derived from pre-existing forms.

But when we find that under certain circumstances the same form of organism originates from these particles, as we should expect were the latter spores, the case is made still stronger. This latter fact will be more evident when we come to consider the relations of these bodies to fermentations, but I shall here mention one experience related by Mr. Lister.[1] In some experiments on the growth of a fungus in urine, a form of micrococcus, which he terms *Granuligera*, constantly appeared in the urine. He found that these were really organisms, from an opportunity which he had of watching their growth. He then says:—'About this time, my study suffered from an epidemic of Granuligera. I could not now perform the same experiments with the same success as in the first instance: any that I tried was sure to be followed by the development of this pervading organism. I eluded it, however, by continuing the investigation in a room at the top of the house, which had been for a considerable time unoccupied. Here the results of the experiments corresponded with those originally obtained in the study.' In this experiment there cannot be the slightest doubt that the particles which gained access to the cultivating fluids were these micrococci or their spores, if they have any. It could hardly have been some form of organic molecule not derived from these organisms which always gave rise to exactly the same forms.

Such are some of the facts which show that it is not unreasonable to look on these particles as bacteria or their germs; and Dr. Bastian, as I have already said, admits that some of the particles are derived from pre-existing bacteria.

II. Secondly, not only is it not unreasonable to regard these particles as bacteria or their germs, but *there is no direct evidence whatever that organic molecules not derived from bacteria can give rise to bacteria.*

[1] See *Transactions of the Royal Society of Edinburgh*, 1875.

Look at all the facts I have brought forward as to unboiled fluids and tissues. I have shown that the most diverse organic fluids and tissues may be preserved for an indefinite period, without the development of organisms so long as the particles in the air and on surrounding objects are excluded. And yet these substances are full of organic molecules derived from previously living structures.

Cut a piece of liver, kidney, muscle, or other tissue from a healthy animal not yet dead, and place it, with precautions to exclude atmospheric dust, in a flask containing cucumber solution. Here you have at first in the fluid and in the tissue *living* cells and *living* organic molecules—and we know, as in the case of muscles, that life may be retained for some time—and yet no organisms appear. These living organic molecules of all kinds do not give rise to bacteria or any other form of life—they die.

Again, when the same specimen has stood for some time, we have a highly putrescible and fermentescible material containing *dead* organic molecules of all kinds; and yet no life appears, and such flasks may be kept for an indefinite time without the development in them of life.

But again, these organic molecules are not only dead, but *decaying*. They undergo slow oxidation—what Liebig has termed *eremacausis*. And yet they do not develop into or induce the appearance of living forms.

Organisms only develop in these fluids and tissues when bacteria or their germs or when atmospheric dust is introduced. Whence, then, are the hypothetical organic molecules derived? If they do not develop life in such fluids as cucumber or meat infusion, or in meat itself, under the conditions described, how is it that they come to do so after having floated through the air? Is it that they have acquired new properties in the air, or is it that the organic molecules (not derived from pre-existing bacteria) which, falling from the air into the fluids or on the tissues, give rise to living forms, are specially manufactured in the air? One or other of these conclusions must be accepted, and I say they are both equally untenable.

And when I come to speak of the relations of organisms to fermentations, more especially to fermentations occurring in wounds, I shall bring forward evidence of a similar character.

Thus I take flasks containing pure, highly putrescible fluids, such as meat or cucumber infusion, or milk, and I introduce into them pus, blood, or serum from wounds, and place them under the most favourable circumstances for the development of organisms, and yet no organisms develop. The same reasoning applies here as in the case where larger living masses, as liver, have been introduced into similar infusions. On the other hand, if bacteria or micrococci are present in the discharge (and I confirm this by microscopical observation), organisms develop in these flasks, and organisms of precisely the same morphological characters as were present in the original discharge.

But let us glance for one moment at Paul Bert's work.

He subjects such substances as saliva, pepsine, myrosin, emulsin, &c., to high pressures, say twenty atmospheres, and he finds that when the normal pressure is restored, these substances have not in any way suffered, as regards their fermenting power.

On the other hand, he places putrefying or fresh meat under a similar pressure, and after restoring the normal pressure, he finds that if he excludes fresh atmospheric particles, putrefaction is in the first case arrested, and in the second, never takes place. Organisms never again develop unless fresh dust is admitted.

From the first set of experiments we see that organic molecules when they have any power of acting are not deprived of it by high pressure, while from the second we learn that under similar circumstances life is destroyed, and no new life appears. And yet this cannot be because the power of generating organisms, which organic molecules are supposed by Dr. Bastian to possess, has been destroyed by the high pressure; for, as we see in the first experiment, dead organic molecules, as distinguished from living ones, retain their powers even under this pressure. The organic molecules, then, which originated the bacteria were living molecules, but *not merely any living molecules*, as is evident from the liver experiments, *but living molecules derived from pre-existing bacteria.*

Then, again, Cazeneuve and Livon's experiments, which will be detailed presently, prove exactly the same points. They show that it is not organic molecules in urine or in the wall of

the urinary bladder which originate bacteria, but that it is organic molecules derived from the dust of the air; in other words, derived from pre-existing bacteria.[1]

All the proof which Dr. Bastian attempts to adduce on this point is to bring forward the opinions of several eminent metaphysicians as to the first origin of life on this globe. But with all respect for these great minds, I do not see one particle of *proof*, but merely metaphysical speculation, in the extracts quoted. Dr. Bastian removes the question to the first origin of life, and at once throws aside the creation view as untenable. I do not intend here to enter on this question, but I for one am not prepared to go so far.

III. Thirdly and lastly, we have *no evidence that active organic molecules can resist heat more than living things can.*

Let us take the so-called 'unformed' ferments, which consist of active organic molecules. The action of saliva on starch is favoured by a temperature of from 35° to 40° C., but its amylolytic activity is permanently destroyed by heat, even below the boiling point, applied for a few minutes.

The same is true of gastric juice; 'at temperatures much above 40° or 50° C. the action of the juice is impaired. By boiling for a few minutes the activity of the most powerful juice is irrevocably destroyed.' (Foster.) And so with pancreatic juice, &c.[2]

I have previously discussed at length the errors in experiment and observation which have led to the belief that organisms can develop in materials subjected to a *moist* heat for a sufficient length of time. And I have quoted Dr. Roberts's refutation of the experiments with alkaline fluids.

I stated on page 193 that I had only met with one instance, in an exceptionally long experience, of difficulty in sterilising an organic fluid. This fluid was milk got from a particular shop near the Strand. When I got fresh milk from

[1] Again, Pasteur by his experiments of opening vessels in different situations has distinctly shown that *all* particles of dust do not cause development of organisms; for undoubtedly organic *débris* did get into all his flasks, especially those opened outside, and yet it was only when certain special particles entered that organisms developed.

[2] Compare also Bastian's views on the *Disruptive Agency of Heat*, referred to at page 190.

a dairy in Albany Street, I had no difficulty in rendering it barren by immersion in boiling water for a few minutes. But the milk from the shop near the Strand has been immersed in boiling water even for three hours, and yet organisms developed in it. It is to be noted that in all cases the bacteria were identical in form and in the effect which they produced on the milk: they were a form of long bacillus. Now here we had some particle which got into the milk and caused a special change in it, and a constant development of a particular and easily recognisable form of organism—an organism which, moreover, has been found by other observers to resist heat, and especially dry heat, in an extraordinary manner. Is it reasonable to suppose that the particles which gave rise to these organisms were organic molecules derived, goodness knows from what, or specially manufactured in the air of this shop? Or is it not more reasonable to suppose that the air was infected with some form of bacterium or its spores, just as in Mr. Lister's case with Granuligera, and that this special form or its spores possessed the same resisting powers which it has been found to have in other parts of the world. For I may add that the spores of bacilli are stated by all who have investigated the subject to be possessed of extraordinary resisting power.

After all it is not a matter of great surprise that an organism should resist a heat of 210° F. (that has been ascertained by Mr. Lister to be the temperature of milk immersed in this way), for Prof. Jeffries Wyman tells us of a form of alga which lives normally in water the temperature of which is 208° F.[1]

With this great mass of evidence, and I could have multiplied it manifold, I do not see that there can be grounds for

[1] Doyères found that dried Tardigrades were not destroyed till the temperature reached 140°.

Payen showed that the spores of oïdium aurantiacum did not lose their germinating power till the temperature reached 140° C.

Milne-Edwards has found that dried organisms could resist a very high temperature.

Instances of great resisting power of seeds to heat have been brought forward by Tyndall and others. Mr. James Sanderson, of Galashiels, tells me that in some specimens of wool obtained from South America, seeds of medicago are present, which develop even after the wool containing them has been dyed—*i.e.* after they have been subjected to prolonged boiling and to the action of various chemical substances.

any longer retaining the view of spontaneous generation. It would just be as sensible to suppose that in the impregnation of the ovum of higher animals it is not the spermatozoa, but some organic molecule accidentally introduced at the same time which causes the development of the ovum, as to suppose that it is not bacteria or their spores but some organic molecule manufactured in or specially altered by passing through the air which produces bacteria in organic fluids and tissues.

CHAPTER XI.

RELATION OF MICRO-ORGANISMS TO FERMENTATION.

Summary of what has preceded with reference to fermentation. Relation between 'vital' and 'chemical' fermentations: theories of fermentation. Liebig's views. Alcoholic fermentation: *Pasteur's experiments and conclusions. Résumé.* Butyric fermentation. Formation of pigment by bacteria *Schroeter: Cohn.* Viscous fermentation. Lactic fermentation: *Pasteur: Lister.* Other fermentations, especially the putrefactive: *Lemaire; Cazeneuve* and *Livon: Paul Bert.* Conclusions.

We must now pass on to the relation of these bodies to fermentations, and I will here merely indicate the chief points without entering into a discussion on the subject And first, I may say that it is now admitted by Dr. Bastian, s well as by other observers, that organisms are present in all fermenting fluids. This statement was formerly denied, on account of the imperfection of the methods of examination.

We have already seen in the first part of this work that the cause of fermentation in organic substances was the entrance into them of solid particles held in suspension in air. We have also seen that the cause of the development of organisms in fluids and tissues was the entrance into them of particles suspended in the air. We also know that in all fermentations organisms are present, and that in the absence of organisms no fermentation occurs. What more likely, then, than that the particles which cause fermentation, and the particles which originate organisms, are one and the same? that in fact the fermentation of a fluid is the result of the growth of organisms in it?

The process in these 'vital' fermentations may be brought into the same category as that in fermentation by the 'unformed' ferments, if we suppose that the immediate cause of the chemical change in the former instance is some chemical

substance resembling ptyalin, pepsin, &c. No doubt there is this difference between ordinary fermentations and those due to a chemical substance, that in the former case the ferment itself increases in quantity. This difference would, however, be easily reconciled if we were to suppose that each organism was a former of the ferment, even though to an extremely limited degree. The process in these 'vital' fermentations, of which we may take the alcoholic as an example, would then be the same as in the so-called chemical fermentations. Thus, to speak of the case of the saliva, ptyalin is not a chemical compound formed spontaneously, nor is it the result of any sort of double decomposition; it is a substance formed as the result of the vital action of certain living cells. According to the view under consideration, the alcoholic ferment would be likewise the product of the vital action of certain cells, the yeast cells. The ptyalin itself has not the power of self-multiplication, but the cells which form it produce it continuously; the ferment of alcohol would not have the power of self-multiplication, but the cells which form it produce it continuously. This explanation agrees completely with the contrast between the effects of ptyalin on starch, and those of yeast on sugar. The ptyalin is more or less immediate in its effect. It is a very active formed ferment. The yeast acts slowly because the ferment is only produced as the plant grows. Hence the explanation of the rapid action of the one and of the progressive action of the other. The apparent self-multiplication of the ferment in the alcoholic case would be due to the fact that the producers of it are free—float freely in the fluid—and hence ultimately no drop of it can be taken which will contain sufficient ferment to act without the ferment-producers being also present. In the case of the saliva, the ferment-producer is fixed, and the ferment is obtained alone and apart from its originator, hence it does not multiply. The same causes which arrest the production of ptyalin arrest also the alcoholic fermentation, for they destroy the living cells which form the ferment.

The case of emulsin would be exactly the same as that of yeast. The emulsin itself does not multiply, but the seed, the producer of it, does. Sow a seed of the bitter almond, and there springs up a plant bearing numerous seeds, numerous producers

of emulsin. Sow a yeast cell, and there follows the growth of numerous yeast cells, each producing a quantity of the ferment.

Considerable support is given to this view by the experiments of Musculus, on the mechanism of the ammoniacal fermentation of urine. This has been shown to be due to the growth of an organism in the urine (Micrococcus ureæ, Cohn). Musculus[1] demonstrated that, by adding absolute alcohol to ammoniacal urine, a precipitate was obtained which could be filtered and dried. This precipitate transformed urea into carbonate of ammonia. Its power was destroyed by exposure to a temperature of 80° C. This soluble ferment is a secretion of the micrococcus ureæ.

According to another view it may be that the organisms, while living in various substances, feed on them, and the products of the fermentation may be either the portion of the food which has been rejected by them, or products formed in and excreted by the organism.

Or it may be that, as Pasteur holds, the cause of the fermentation is not the production of a ferment but the breaking up of the chemical compounds by the growing plants in the search for nutriment, more especially for oxygen. It is quite clear that there must be such a breaking up. Or, again, it may be that in this breaking up of the organic compounds some of the molecules may rearrange themselves and form a ferment, and the presence of a ferment of this kind I consider the best explanation of some of the special fermentations, though I incline to hold that the ferment is excreted by the cell itself.

It is, however, probable that in different fermentations the process occurs in different ways. Thus in the pigment fermentations, as will be seen, the second is probably the correct explanation. The point of importance is that in any case there is nothing unreasonable in associating these changes with the growth of living cells; in fact all analogy points to such a relation.

The only other theory which is tenable in the presence of the facts stated, would be that the particles which cause fermentations are not the same as those which give rise to the growth of organisms, but that they are bodies which have only the

[1] Magnin, *Bacteria*

power of causing fermentation, and are possibly either substances in a state of decomposition or special ferments. But then it is inconceivable that fermentation and the development of organisms should always be associated, or that organisms of the same form, *e.g.* the yeast cell, should always be present in the same fermentation. Supposing this view possible, we must assume that there is a special organic molecule for each fermentation, for otherwise we could not explain the occurrence of lactic fermentation in one flask, putrid fermentation in another, pigment fermentation in another, &c., all the flasks containing the same specimen of milk having been filled at the same time and kept under the same conditions.

Liebig originally propounded the view that fermentation was a change in organic fluids and tissues, set in motion by the access of oxygen or of bodies in a state of decomposition. He at that time regarded organisms as quite accidental. He supposed that when organic matter was exposed to the air, it underwent a slow process of oxidation which he termed *eremacausis*; and that this change, communicating itself to other molecules, caused them to break up or putrefy. In the presence of the facts stated as to the relation of dust to the fermentation of boiled and unboiled fluids, Liebig modified his views, and in his last publication he admitted that the yeast plant was in some way or other connected with the alcoholic fermentation, but he thought that the relation between them most frequently consisted in this :—that when the yeast cells died, they decomposed, and that the chemical change thus set agoing was propagated to the sugar, and caused it to break up into alcohol and carbonic acid.

That Liebig's theory of decaying matter is incorrect will be seen by a consideration of the facts mentioned on pages 200 and 210. Liebig was not latterly, however, absolutely opposed to the acceptance of the doctrine that living organisms are initiators of fermentative changes. On the contrary, to quote from Bastian, 'he slightly widened his views after the correlation of organisms with fermentations had become established, and endeavoured to show that the admitted actions of living units in initiating fermentations were but other exemplifications of his general doctrine, that fermentations are induced by certain communi-

cated molecular movements, sometimes emanating from organic matter in a state of decay, and sometimes resulting from the vital processes of living units.'

Had Liebig left out the part of the view which holds that fermentations may be caused by movements, 'sometimes emanating from organic matter in a state of decay,' there would not have been much to find fault with. His theory would then have been merely another way of viewing the mode of action of these living bodies.

Certain definite facts are known which show that organisms do take part in certain fermentations, while I have already disproved Liebig's view that decaying matter has any power of causing these changes. The facts to which I am about to allude, when taken together with the constant presence of organisms in other fermentations of the same class not yet investigated, render it, to my mind, certain that living organisms are, probably in one or other of the ways indicated, the causes of these chemical changes.

Alcoholic Fermentation.

The first case of fermentation which was studied, and about which most is known, is the alcoholic fermentation. The facts and experiments on this subject are now so universally known that it would be superfluous for me to do more than summarise them here.

The yeast plant (*Torula cerevisiæ*) is always present in a state of vitality during the alcoholic fermentation of sugar.

If yeast be raised to the temperature of 60° C., at which temperature the cells die, the fermentation of sugar no longer occurs. Here the cells are dead, and if Liebig's view be correct, that dead cells, not living ones, are the cause of the fermentation, the process ought still to go on.

The juice of the grape has no spontaneous tendency to undergo fermentation, as shown by the experiments of Van der Broeck, &c., formerly mentioned, and by the following experiment narrated by Pasteur. In a flask of the form shown in Fig. 68, A) the neck (*a*) of which was drawn out to a fine point, Pasteur boiled water which had been used to wash the outside

of a grape, and which therefore, according to other experiments, contained abundant causes of the alcoholic fermentation. This point (*a*), which had been sealed, was heated, and plunged through the heated skin of a grape (Fig. 68, B). It was then broken in the interior of the grape, and, by causing a diminution of pressure inside of the flask, a drop or two of the juice was sucked into the boiled water. (The diminution in pressure was thus obtained. After heating the walls of the flask with the hand or a lamp, the orifice of the bent neck (Fig. 68, A, *b*) was sealed. When the point of *a* was broken off in the interior of the grape a little juice passed into the flask.) The orifice *a* was then sealed, and the orifice *b* opened. Here, then, he had in the flask unboiled grape juice, oxygen, water, dead organisms, and organic mole-

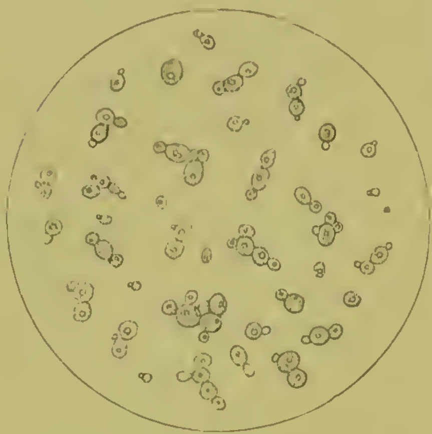


FIG. 67.—TORULA CEREVISIÆ. (AFTER PASTEUR.)

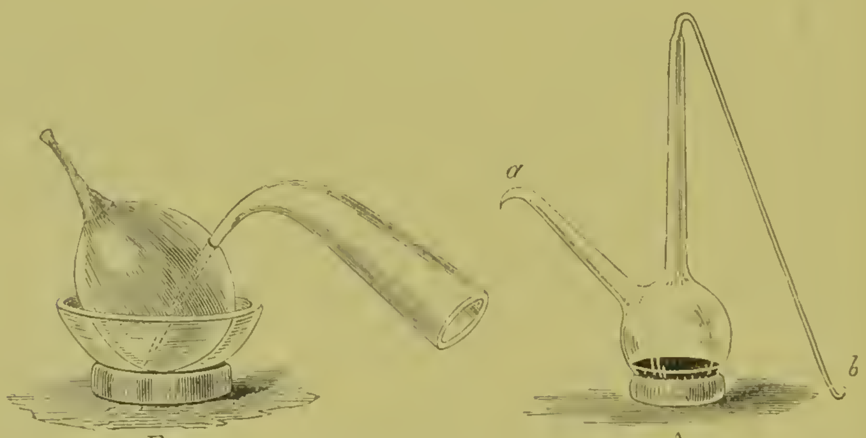


FIG. 68.—PASTEUR'S EXPERIMENT ON UNBOILED GRAPE JUICE.

A, the flask employed. *a*, The neck which is represented in B as plunged into the grape. *b*, The orifice of the bent neck at first sealed till the grape juice has been introduced and then opened and left open. (From Pasteur.)

cules, and yet no alcoholic fermentation occurred. This and other experiments[1] led Pasteur to the following conclusions :—

[1] See *Etudes sur la Bière.*

'The boiled juice of the grape never ferments when kept in contact with air which has been deprived of the germs suspended in it.

'Boiled grape juice ferments when a very small quantity of water is introduced, in which the surface of the grapes and of the branches of the vine have been washed.

'Grape juice does not ferment after the introduction of this water if the latter has been previously boiled and cooled.

'Grape juice does not ferment when a small quantity of the juice taken from the interior of a grape is introduced.'

Pasteur further shows that the apparently spontaneous commencement of fermentation in these fluids is due to the existence of spores of the torulæ in the air, though they are not, as a rule, present in great abundance. He also brings forward experiments to show how other fungi, such as *Mycoderma aceti*, can, when there is too little oxygen present, cause the splitting up of sugar into alcohol.

Seeing, then, that boiled and unboiled grape juice may be preserved unaltered in presence of air which has been heated, or in some way or other deprived of its dust, it is quite evident that alcoholic fermentation is not a spontaneous decomposition nor one which can be brought about by the action of the gases of the air alone. Seeing, also, that the yeast plant is always present when fermentation is going on, that anything which destroys the vitality or interferes with the growth of that plant arrests or interferes with the alcoholic fermentation, and that the introduction of dust which does not contain the torula cells is not accompanied by the alcoholic fermentation but by some other change, we must conclude that the particles which fall into fluids and give rise to alcoholic fermentations are intimately associated with the particles which give rise to yeast cells, and further that they are living particles subject to the same laws as the yeast cell itself. And seeing, further, that other plants when living under certain conditions are also capable of giving rise to the alcoholic fermentation, we must conclude that this change is a process due to changes in living tissues, that, in fact, the fermentation is the result of the life of these cells and plants, and that, therefore, the particles which, falling into fluids, give rise to the Torula cerevisiæ, and those which give

rise to alcoholic fermentations, are one and the same; in fact that the growth of the yeast cell is in some way or other the *cause* of the alcoholic fermentation.

Butyric Fermentation.

Pasteur has also brought forward evidence of a similar nature as to the butyric fermentation. The organisms which produce butyric fermentation are bacilli (*Bacillus subtilis*, Cohn), which apparently live without free oxygen, and indeed are killed by it, and which when cultivated in various fluids, even in Pasteur's solution, cause butyric fermentation in all (see Fig. 69 and also Fig. 64).

FIG. 69.—BACILLUS SUBTILIS, × 650. (AFTER COHN.)

Formation of Pigment by Bacteria.

Striking facts as to the association of a definite chemical change with the presence of organisms of a definite form were brought forward by Cohn and Schroeter with regard to 'pigment bacteria.'[1]

They showed that while many forms of organisms could grow on such soil as boiled potatoes, yet a definite pigment was produced only when one particular form was present. These organisms generally belonged to the group of 'micrococci,' though sometimes pigmentation was caused by bacteria, as in blue pus (see Fig. 70).

These pigments were sometimes scarlet, sometimes blue, sometimes soluble, sometimes insoluble; and when a variety of soils were inoculated from an individual specimen, the same colour, with the same chemical and other characteristics and the same organism, always resulted. Not only might these pigment bacteria grow on boiled potatoes, they could flourish

[1] *Beiträge zur Biologie der Pflanzen.*

on cheese, meat, white of egg, bread, starch, &c., the same pigment being invariably produced.

The conditions under which this pigment appeared were exactly those which were most favourable to the life of the organisms, while those in which it was absent were those in which the organisms could not develop.

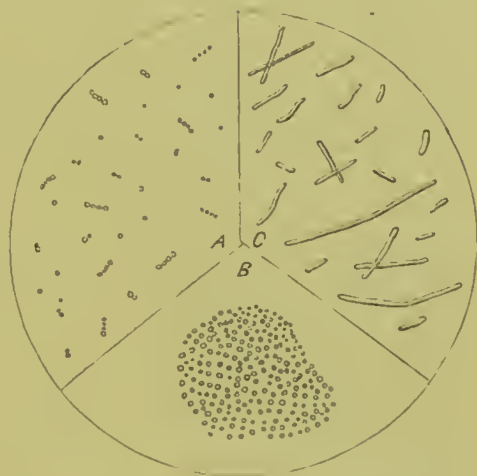


FIG. 70.—PIGMENT PRODUCING ORGANISMS.

A, Micrococcus prodigiosus. B, Micrococcus fulvis. C, Bacillus ruber.

Schroeter concludes from his investigations that these examples show what a manifold series of pigments may be produced 'by bacteria and bacteridia.' He adds that the organisms which form them can often be recognised as distinct owing to the difference in the pigment produced; the organisms which form the various pigments being often also distinctly separable by different morphological characters, and the different pigments behaving differently with reference to chemical reagents. He considers that it is not unjustifiable to hold that each separate pigment is formed by a distinct specific organism.

Schroeter points out that the pigments are definite chemical substances formed by the bacteria from organic, albuminous materials, and that the process is therefore quite analogous to the formation of alcohol by the yeast plant or of lactic acid by other bacteria.

Cohn further found that these same organisms developed readily and produced the same pigment in *artificial cultivating fluids containing ammonia and a carbonate, but no trace of albumen*. Once obtained in this fluid they could be propagated indefinitely, the same pigment being constantly produced. In this instance we have an example of a definite change brought about by the growth of a definite form of organism. No *spontaneous* change ever occurs in Cohn's artificial fluids resulting in the formation of these pigments, and yet as soon

as these organisms, which are associated with definite pigments when growing on albuminous soil, are introduced into these mineral solutions, the same pigments appear. That in the latter case the pigment is formed by the bacteria is evident, for it is a complex organic substance, closely related to the aniline colours, and yet in this instance it is derived from a few inorganic salts. It is therefore a substance formed by the living plants, and, as before remarked, there is nothing remarkable in this production of pigment by living cells. It is only what is constantly occurring in many animal and vegetable cells, and in the latter it is formed from inorganic compounds. Indeed, it would be against all chemical experience that by any purely chemical process a complex organic compound would be so readily built up from a few inorganic compounds of a very simple kind.

These facts absolutely prove that the pigment fermentation was the result of the *life* of the organism, for the pigment must have been built up by it and could not have originated from chemical changes propagated to the fluid. If then Dr. Bastian is correct in his opinion that 'Liebig's view, if it be true at all, must be true for *the whole* of the processes which are essentially included under the term fermentation,' these facts alone overturn Liebig's theories.

Viscous Fermentation.

Similar facts are known with regard to the viscous fermentation of sugar. This is a transformation of sugar into gum mannite, and carbonic acid, and results in the formation of a viscid, ropy fluid. Pasteur found that organisms of a special form were present in such instances, and these organisms when sown in Pasteur's solution always caused this viscous fermentation. The kind of proof is exactly the same as in the other instances of fermentation, and need not be repeated here.

Lactic Fermentation.

An instance in which the causal relation of organisms to a fermentative process is absolutely demonstrated is the lactic fermentation of milk.

This fermentation was investigated several years ago by

Pasteur,[1] who showed that it was exactly analogous to the alcoholic fermentation, and that it was caused by a special organism. On examining fluids which were undergoing lactic fermentation he observed that minute round or oval cells were constantly present. These organisms when transferred to fluids capable of nourishing them always produced lactic acid. To prove that these organisms were the cause of the fermentation he made a decoction of yeast, to which he added sugar and chalk. When they were sown in this fluid lactic fermentation occurred, resulting in the formation of large quantities of calcium lactate.

This fermentation has of late been more fully studied by Mr. Lister, and as his experiments bear on the whole subject of fermentation, I shall refer to them somewhat at length.

As I have previously mentioned, Mr. Lister[2] found that milk had no inherent tendency to undergo the lactic fermentation; in fact, unless it was brought into a dairy or into contact with dairy vessels or workers, all sorts of fermentations would occur rather than the lactic. On examining milk undergoing lactic fermentation he found that an organism of a definite and easily recognisable form was constantly present, while in milk which was not brought into the dairy, and which did not undergo lactic fermentation, this form of organism was absent.

'This organism is a motionless bacterium occurring most commonly in pairs, but frequently in chains of 3, 4, or more individuals, each segment being of somewhat rounded form, more or less oval, with the long diameter in the direction of the length of the chain, and often showing, on careful focussing, a line across their central part, indicating transverse segmentation. They vary in diameter. . . full-sized specimens measuring about $\frac{1}{20000}$th inch.'

The question now arose, Was this organism, so constantly present in milk which was undergoing the lactic fermentation, the cause of that change or not? Was some other bacterium the active agent, or was it some hypothetical organic molecule?

Mr. Lister solved these points in the following beautiful manner. He prepared a series of flasks containing milk which

[1] *Annales de Chim. et de Phys.*, vol. lii. p. 407.

[2] *Transactions of the Pathological Society of London*, 1878.

had been boiled. These were kept for some time till it was certain that the milk had been rendered sterile. Having calculated how many of these oval organisms were present in a given quantity of fermenting milk, he diluted this milk so as to have only one bacterium in a definite quantity (*e.g.* $\frac{1}{100}$th of a minim) of the fluid, supposing that the bacteria were equally diffused throughout it.

This was done in the following manner :—'By means of the syringe already described' (one graduated to the $\frac{1}{100}$th of a minim) 'one or more hundredths of a minim could be measured with precise accuracy; and I found that $\frac{1}{50}$th minim exactly occupied a circular plate of thin covering glass, half an inch in diameter, so that when such a drop was placed on a glass slide, and a cover glass of the size mentioned and quite flat was put down upon it, all air was expelled from under the latter, and the rim of fluid that formed round about its margin was so narrow as not to measure a quarter of the diameter of the field of the microscope even when the highest magnifying power was used. In other words, $\frac{1}{50}$th minim was disposed in a thin uniform layer of the exact size of the cover glass. Hence the number of bacteria under the glass slip—that is to say, in $\frac{1}{50}$th minim—was equal to the number of the bacteria in a field of the miscroscope multiplied by the number of times the area of that field went into the area of the covering glass. The micrometer gave the diameter of the field in thousandths of an inch; and the cover glass measured 500 thousandths of an inch across; and the areas of the circles were of course proportioned to the squares of those diameters. All that was needful, therefore, in order to enable me to calculate the number of bacteria in $\frac{1}{50}$th minim, was to form a fair estimate of the number of bacteria per field, and this was done by counting the organisms in a considerable number of fields, and taking the average.

FIG. 71.—BACTERIUM LACTIS IN PAIRS AND CHAINS.

In one chain the component cells are undergoing division. (After Lister.)

'As the result of the estimate which I made of the number of bacteria present in every $\frac{1}{50}$th minim, I found it necessary to dilute the milk with no less than a million parts of boiled water, in order that every $\frac{1}{100}$th minim should contain on the average a single bacterium.'

Having obtained the necessary dilution Mr. Lister proceeded as follows :—'One-hundredth minim of the infected water was added by means of the syringe to each of five glasses of pure boiled milk. The result of this inoculation was that only one of the five glasses was affected at all.' The others remained unchanged, without fermentation, and without bacteria development. The one which was affected underwent lactic fermentation, and in it the bacterium lactis alone was found, no other form of organism was present. This bacterium was inoculated into urine and developed there. After four days milk was inoculated from this urine. The milk underwent lactic fermentation, and these bacteria were again found. Drops of urine, diluted so as to contain three bacteria per drop, caused lactic fermentation in all the vessels to which they were added.

The following experiments afford absolute proof that the bacterium was the cause of the fermentation :—

'On August 30 last (1877), having provided sixteen pure glasses of boiled milk, and having estimated, in the manner already described, the number of bacteria present in every $\frac{1}{50}$th minim of a glass of boiled milk, which had been inoculated the day before by touching it with a heated needle dipped in milk curdled under the influence of the pure ferment, I diluted a drop of this milk with boiled water to the requisite degree, and introduced into each of ten of the sixteen uncontaminated glasses a drop calculated to contain on the average a single bacterium, while five of the rest received each a drop supposed to contain two of the organisms, and the remaining glass was inoculated with a quantity in which, according to the estimate, there would be four bacteria. The result was that within three and a half days the glass into which four bacteria were supposed to have been introduced contained a curdled mass, and the five which had received the drops arranged for two bacteria each had all undergone a similar change. Of the ten inoculated with drops averaging one bacterium each the majority were at this period still fluid, but some assumed the solid condition in the course of the next twenty-four hours, though at different times. But of this series of ten, exactly five, as it so happened, remained permanently fluid.'

Every glass in which curdling had occurred contained the

bacterium lactis; the five glasses in which the milk was unaffected contained no organisms.

Hence it seems clear that when this organism is present in milk lactic fermentation occurs. Where it is absent this change does not take place, for, as Mr. Lister argues, we could hardly suppose that an organic molecule or ferment would occur exactly in the same cases as the organisms appeared, unless there was some intimate relation between them. If organic molecules, independently of the organisms, were the cause of this fermentation, some flasks ought to undergo lactic fermentation without the presence of any organisms; others ought to show development of these organisms, but no lactic fermentation.

Other Fermentations, especially the Putrefactive.

I may just refer more as a matter of historical interest than of real use in this question to Lemaire's experiments with carbolic acid and his opinions on fermentation.[1]

Lemaire showed that the addition of carbolic acid to organic fluids and tissues prevented putrefaction and other fermentations. Carbolic acid, according to him, did not interfere with the fermentations caused by 'unformed' ferments, such as synaptase, &c.

He then pointed out that the unformed ferments can act at temperatures at which the other ferments are inert, as, for instance, at zero and at 70° C. Trituration of yeast destroys its fermenting power, while trituration of emulsin does no harm. In, fact anything that favours life favours alcoholic and allied fermentations, while anything which is inimical to life is also inimical to these fermentations, though many of these things do not interfere with the action of 'unformed' ferments.

I have before referred to the experiments of Cazeneuve and Livon on unboiled urine.

The method of obtaining the bladder with its contained urine has been previously described; and in the successful experiments formerly mentioned on p. 37, no organisms were

[1] *L'acide phénique*, 1865.

found, while if the somewhat concentrated urine were removed and diluted with ordinary water it became alkaline in twenty-four hours, and filled with 'torulacée.' Results similar to those mentioned were obtained when the urine had been previously rendered alkaline by the administration of soda or potash.

Puncture of the bladder was soon followed by alkalinity and development of organisms in the urine: hence it is not the absence of oxygen from the urine which is the cause of the absence of change in it. The following experiment shows that the merest trace of oxygen is all that is required, if indeed it be at all necessary.

Prevent the evaporation through the walls of the bladder, by immersing it, immediately on its removal from the body, in melted paraffin at the temperature of 45° C. This temperature is insufficient to destroy the germs which fell on the wall of the bladder during its transit from the abdomen to the paraffin.[1] Thus a layer of paraffin covers the outside of the bladder, preventing the rapid evaporation of the fluid which exudes while living organisms are present on the wall of the bladder. In twenty-four hours remove the paraffin case. It is then found to contain an alkaline turbid fluid full of organisms. These organisms have not, however, had time in twenty-four hours to penetrate into the interior of the bladder, and therefore the urine inside is found to be still acid and devoid of life. *The same is the case with urine rendered alkaline*—the fluid outside contains organisms, that inside is free.

But let the bladder be first dipped in paraffin at 100° C., so as to destroy any living organism in contact with the wall, and then, after removing it from this paraffin at the end of a minute, let it be plunged into paraffin at 45° C., so as to get a thicker coat (this paraffin is previously heated to 110° C., and during cooling is protected from the dust), it will be found that even after three days the fluid outside the bladder—in the paraffin cup—is still clear, acid, and devoid of organisms. Leave this bladder now exposed to the air for say five hours, then give it a new coating of paraffin at 45° C., and leave this on

[1] The reason why the organisms do not develop on the bladder hung up in the air is that the fluid dries as soon as it exudes, and therefore the organisms have no fluid in which to develop.

for three days. The fluid outside the bladder will be found in this case to be ammoniacal and to contain organisms. Hence the walls of the bladder and the fluid in the interior were not modified by the heated paraffin in the first part of the experiment.

These experiments alone are sufficient to refute Liebig's view of organic molecules and decaying matter ; for in the first part of the experiment they were present in an unlimited amount, but so long as organisms were excluded no fermentation occurred. Their bearing also on the theory of spontaneous generation will be at once evident, and has indeed been already alluded to. I would only refer here to the experiments in which the urine was made *alkaline*, and in which therefore we had a natural alkaline urine full of organic molecules and in contact with the tissues of the bladder, and yet no organisms appeared in it so long as the dust of the air was excluded ; in other words, *the alkali had no influence in determining the re-arrangement of the numerous organic molecules which were present in the wall of the bladder and in the fluid in its interior*, *so as to form new living beings*. This is in exact correspondence with Dr. Roberts's results, and is a much more telling experiment.

Some very remarkable and convincing facts have been lately obtained by Dr. Paul Bert.[1]

On subjecting the 'unformed' ferments, such as ptyalin, pepsin, inversive ferment, myrosin, and emulsin to high degrees of pressure, he found that the properties of these ferments were not in any way impaired.

Thus to quote Experiment 467 :—

'21 Juillet 1874. Saliva humaine étendue d'eau et placée dans un matras étiré à la lampe, et soumis à 15 atm. d'un air suroxygéné.

'Le 30 Juillet je décomprime et soude l'extrémité du tube effilé.

'18 Janvier 1875. Cette saliva qui ne sent rien et paraît bien normale, neutre aux réactifs, transforme avec une grande énergie l'amidon cuit en glycose.'

It was proved by a former experiment (p. 201) that the amylolytic property of ptyalin was not altered by diminution of pressure, and the same is true when the pressure is increased :

[1] *La Pression barométrique.*

and, what is of great importance, other ferments of the same class, which are apt to lose their properties when kept, owing to the occurrence of putrefaction, retain these after being subjected to strong pressure if new causes of putrefaction are excluded. The explanation of this fact is simply that the bacteria and fungi are killed by the high pressure.

Bert also enquired whether these ferments could continue to act, in this compressed air, and he found that though they did continue to act the rapidity of their action was manifestly diminished. Thus Exp. 470:—

'20 Janvier. Salive, amidon cru et eau. Bien mêlé et placé dans plusieurs tubes. On s'assure que le mélange ne contient pas de glycose.

'A—à la pression normale, bouché avec cornet de papier renversé.

'B—à 21 atm. d'air suroxygéné.

'Tous les deux sont mis à l'étuve, 30 degrés.

'25 Janvier. Essayé avec liqueur bleue :—

'A—7cc en réduisent 35 gouttes.

'B—7cc en réduisent 14 gouttes.'

This result was, however, not obtained unless the fluids were examined within a few days. At a later period, especially if diastase was employed, the contrary was found; the fluid, subjected to compression, containing more sugar than the other. It was found that the explanation of this was that the diastase which had remained exposed to the air had become altered, owing to the growth of organisms in it, while organisms being unable to develop in that subjected to high pressure, the diastase had retained its properties and continued to act.

Paul Bert concludes: 'All the soluble false ferments with which we have experimented, diastase, ptyalin, pepsin, inversive ferment, myrosin, emulsin, have yielded the same result, and have retained their characteristic properties, after the prolonged action of oxygen, at a high pressure. Indeed, as this compressed oxygen destroys the germs of fungi, vibriones, &c., which sooner or later destroy these ferments when exposed to ordinary air, the latter remain unaltered for an apparently indefinite period of time.'

If we now compare these facts with those obtained by subjecting the 'true' ferments—those causing putrefaction, &c.—to

high pressures, we shall find a remarkable contrast; the latter class of ferments behave under high pressures like living beings, not like the unformed ferments just mentioned.

Bert says: 'The most striking fact which has been made out in these experiments is that, in air sufficiently compressed, putrefaction does not occur, no disagreeable odour manifests itself, and muscle, for instance, preserves its normal appearance except in colour; its microscopic structure is not markedly altered.'

Thus Exp. 404. '17 Mars. Viande en morceaux et eau; dans 2 petits matras effilés à la lampe. A—à la pression normale.

'B, B′—à 16 atmosphères d'une compression faite avec de l'air contenant 80 pour 100 d'oxygène.

'26 Mars. Décomprimé. A, pourri, infect. B n'a pas d'odeur et est neutre aux papiers réactifs.'

But this is not all; for when one restores the pressure to the normal, taking sufficient precautions to prevent the entrance of new organisms from without, putrefaction no longer occurs, and unboiled meat may be preserved at the normal pressure for an indefinite time after being subjected to high atmospheric pressures. The precautions required to prevent the entrance of organisms after the compression, and to ensure the complete destruction of those present, are detailed on p. 184.

Exp. 407. '20 février. On met dans 15 tubes 15 morceaux de viande pesant chacun 1 gr. Ces tubes sont ensuite étirés à la lampe et soumis dans l'appareil en fer, à 15 atmosphères très-suroxygénés.

'3 Mars. On décomprime avec précaution et l'on ferme à la lampe les 15 tubes. L'analyse de 3 d'entre eux, faite aussitôt, donne de 70 à 80 pour 100 d'oxygène.

'13 Mars. 'On brise un des tubes sous le mercure: viande ambrée, pas d'odeur, réaction acide. On trouve 6·2 pour 100 d'acide carbonique et 77·8 d'oxygène.'

From a large number of experiments Bert finds that a pressure of twenty-one atmospheres is sufficient to kill the organisms which cause putrefaction. Similar facts were made out as to fermentations in blood, eggs, urine, milk, alcoholic fermentations, &c. A fact which he observed more especially in connection with blood and milk, is worth mentioning. He found that in the case of milk, as in the case of other substances,

putrefaction was arrested by compressed air. But if *tubes* were used neither coagulation nor rapid acidification were prevented. Was this because oxygen in tension was without action on the bacterium lactis? or was it that the coagulation of milk was not the work of these microscopic organisms, but rather of some agent which can resist oxygen, as we have seen to be the case with the 'unformed' ferments? On further investigation, however, he found that the result depended on the thickness of the layer of liquid. If it was thin the tendency to coagulation was destroyed.

Exp. 431. '10 Août. Lait bouilli; mis en couche de 2 à 3 millimètres d'épaisseur dans deux cristallisoirs neufs et bien lavés.

'A—à l'air libre, sous un verre qui arrête les poussières.

'B—à 25 atmosphères d'air suroxygéné.

'14 Août. Décomprimé.

'A est coagulé depuis le 11 et sent très-mauvais.

'B est liquide, ne sent aucune odeur et paraît normal.'

Paul Bert sums up his results with milk as follows: 'These experiments prove in a very conclusive manner that oxygen in a state of high tension prevents the coagulation of milk, that is to say, kills the vibrios which cause the lactic fermentation. As the action of these organisms is very rapid, it is necessary, in order to arrest it, to employ oxygen at very high pressures, and to have the fluid in a thin layer, so that the oxygen can saturate it quickly. In the case of putrefaction, which occurs much more slowly, these excessive precautions are not necessary; milk differing from blood in not consuming the oxygen as it penetrates the liquid, the gas has time to reach the bottom of the tubes and to kill all the putrefactive agents present. This fact explains how one can so easily, by means of compressed air, prevent milk from putrefying, and yet have so much difficulty in preventing its coagulation.'

It would be superfluous to pursue the proof of this matter further. The greater part of the preceding portion of this work has consisted of evidence which, taken together, can leave no doubt on the mind that putrefaction, like other fermentations, is caused by the growth of organisms in the putrefying material. Pasteur's researches have led him further, and caused

him to adopt a theory of fermentation which has certainly many facts to support it, though I doubt if it can hold good in many instances. He thinks that when a substance putrefies two classes of microscopic organisms are at work, the first in point of time being chiefly engaged in abstracting the free oxygen from the material, and the second, which then appear, being unable to live in free oxygen, but nevertheless requiring oxygen for their growth, and obtaining it from the chemical combinations present. The result of this extraction of oxygen is the breaking up of these compounds—their putrefaction—and the rearrangement of their elements to form new compounds, which constitute the products of putrefaction. Whether Pasteur's theory be true or not, all the experimental results taken together, as well as the numerous facts known as to the power of antiseptics in arresting this class of fermentations, render it no longer doubtful that the particles which fall into organic materials and cause fermentations there, are the same as those which, falling on the same substances, give rise to the lower forms of organisms; in other words, are bacteria or their spores.

CHAPTER XII.

RELATION OF MICRO-ORGANISMS TO THE FLUIDS AND TISSUES OF THE LIVING BODY.

Proposed mode of enquiry—Does the aseptic method prevent putrefaction? Does it exclude organisms from wounds? Ranke's results: Klebs' objection: Ranke's reply: Demarquay: Fischer: Schüller: my own method—Results in aseptic wounds—Results in wounds treated otherwise—Koch's method of staining pus—Results in cases not treated aseptically—Examples of complete exclusion of organisms in aseptic cases—Examples of the entrance of micrococci in aseptic cases—Definition of micrococci—Distinctions between micrococci and bacteria. Are organisms present in the healthy living body?—'Bistournage.' Are organisms present in the body in states of disease?—Experiments with ammonia, phosphorus, &c.—The healthy blood and tissues can destroy organisms—Relation of organisms to abscesses. How do micrococci enter aseptic wounds? Carbolic lotion a sufficient germicide; Spray, its value—Stimson's experiments; Gauze dressing as a protection against entrance of organisms; Carbolic acid as a germicide in albuminous fluids; Relations of micrococci and bacteria to fluids containing carbolic acid. Conclusions.

All the experiments which have been referred to as yet relate to fluids and tissues removed from the body and preserved in flasks. It now remains, before quitting the subject, to enquire if our conclusions hold good for fluids and tissues retained in connection with the living body. An investigation of this sort has been demanded by some writers, as, for instance, by Mr. Holmes. At least that is what I take him to imply by the following passage[1] (I can see no other meaning in it): 'When we are told that, in order to practise antiseptic surgery, we must believe in the germ theory, then I cannot but say that belief is not a voluntary act; it must follow upon proof, and no convincing proof of the germ theory as applied to *living tissues and living phenomena* has, as far as I know, yet been offered.' Granting that I interpret Mr. Holmes' meaning aright, I venture to differ from him as to the necessity for such

[1] See MacCormac's *Antiseptic Surgery*, p. 51.

facts, but nevertheless I think it well to introduce here some investigations which I have carried on with reference to this question.[1]

The mode in which I propose to ascertain whether the former conclusions apply to the living body or not is by the examination, in various ways and under varying circumstances, of fluids and tissues in the body.

We have before seen that the method which we have agreed to term the aseptic method of treatment is nothing more or less than a series of experiments on the germ theory of putrefaction—experiments made with the object of rendering atmospheric dust inert before it reaches the wounds. We shall attain the object of the present enquiry by ascertaining how far these experiments are successful. In discussing this question the following points suggest themselves:—

Does the aseptic method of treatment prevent putrefactive or other fermentations in the discharges or tissues of wounds?

If putrefaction is prevented are organisms also excluded?

If under any circumstances organisms do enter wounds so treated, what are the peculiarities of these bodies?

Are organisms present or do fermentations occur in fluids or tissues in the living body, which have never been exposed to the atmospheric dust?

If organisms are present, how is their occurrence to be explained?

If organisms enter wounds treated aseptically how do they get in?

1. First, then, does this method prevent putrefaction? Undoubtedly it does. Compare the course of an abscess, connected with diseased bone, opened and kept open without aseptic precautions, with that of one opened in accordance with strict aseptic principles. In the former case the pus rapidly undergoes fermentation, in all probability putrefaction; in the latter case the discharge does not undergo fermentation, and remains sweet and pure till healing is complete, however long that may be. I have at this present moment in my mind such a case. A patient, a young woman, came under Mr.

[1] For many details not mentioned here, see the *Transactions of the Pathological Society* for 1879.

Lister's care in August 1876, with spinal disease and psoas abscess. Subsequently a psoas abscess appeared on the other side, and later a lumbar abscess also. I dressed the case almost from the first myself, and though these dressings were changed at first daily and then ultimately weekly for nearly four years (for complete healing did not occur till June 1880, though there had been, for a long time, only minute sinuses furnishing almost no discharge), yet neither putrefaction nor any other fermentation ever occurred in the discharge from these wounds. Indeed, I may say, from long experience of Mr. Lister's practice and from long use of his method myself, that one who has had some experience in this method may now reckon with certainty on avoiding putrefaction or other fermentative change in discharges from any wound made in a situation where aseptic dressings can be applied, provided always that the treatment be strictly carried out by the method described at length at the beginning of this work.

2. Such being the facts with regard to the absence of putrefaction, is it equally the case that organisms are absent from the wounds? We saw a constant relation between the bacterium lactis and the lactic fermentation: can similar facts be found with regard to aseptic and septic wounds?

The first communication on this subject was made by Dr. Ranke,[1] of Halle, in 1874. He published a note of 300 examinations of the discharge from fifteen wounds treated aseptically, and following an aseptic course, in which he states that on only one occasion did he fail to find organisms. His method was simply to look at the discharge through a microscope, and it was not a particularly high power which he employed. The organisms which he says were present were for the most part micrococci in pairs, also streptococci; more rarely small or middle-sized bacteria. He did not carry his investigations farther, but on this evidence he rejects the germ theory as sufficiently explaining the etiology of septic diseases.

While by some these observations have been regarded as accurate and as confirming their previously formed views, by many they have been looked on as erroneous, either from having been made on cases in which the aseptic method had been

[1] *Chirurg. Centralblatt*, No. 13, 1874.

imperfectly carried out, or in themselves faulty. In answer to objections of the former nature urged by Professor Klebs[1] of Prague, Dr. Ranke[2] published another paper in July 1876, quoting cases to show that the treatment had been in reality properly carried out. He instances especially cases of hydrocele, treated by making a small incision into the sac with aseptic precautions and stitching it to the skin, where cure followed without any inflammation or constitutional disturbance, but where, nevertheless, organisms were present in the discharge. From those cases, as well as from the various published reports of the results of Professor Volkmann's practice, there seems no reason for doubting that the observations were made on wounds treated with all due precautions, and following an aseptic course similar to that which Mr. Lister himself would expect.

About the same time Demarquay[3] published the results of eight cases treated 'antiseptically,' in all of which organisms were found. The general course of the wounds so treated, as described by the author, and the fact that one of the eight cases died of pyæmia, show that whether the cases were treated *antiseptically* or no, they were not treated *aseptically*.

Two years later there appeared a paper by Dr. Fischer of Strasburg,[4] giving the result of investigations carried on in Professor Lücke's wards. He employed chemical tests, especially acetic acid and glycerine, as recommended by Von Recklinghausen, and he found organisms in all the cases examined. He, however, states that bacteria were not unfrequently present, his results differing in this respect from those of Dr. Ranke. Now, it so happens, I spent the summer of 1876 in Strasburg, and thus had frequent opportunities of seeing the 'aseptic practice' in that hospital, and I can only say that I was not surprised when I heard that bacteria had been found in the wounds.

The last paper on this subject was published by Dr. Schüller[5] in the spring of 1877. In his investigations at-

[1] *Archiv für Experimentelle Pathologie*, Bd. iii. p. 315.
[2] *Deutsche Zeitschrift für Chirurgie*, Bd. vii. p. 63.
[3] *Comptes-Rendus*, 1874.
[4] *Deutsche Zeitschrift für Chirurgie*, Bd. vi. p. 320.
[5] *Ibid.* Bd. vii.

tempts were made to cultivate organisms from wounds. He found that in many cases organisms were absent both from the discharge and the cultivating liquid, whilst in other cases they were present. He does not specify what the nature of these organisms was, and he is inclined to associate their presence in wounds with the occurrence of tension, &c. There are various objections to his results, but these I need not stay to discuss.

As long ago as 1876 I began a series of investigations on this matter. My first observations were of the same nature as those made by Dr. Fischer; that is to say, I not only examined the discharges microscopically, but I also treated them with acetic acid and glycerine. These substances are recommended by Professor Recklinghausen for this purpose, the glycerine being supposed to dissolve the fat granules and the acetic acid to render the protoplasm invisible; thus, only nuclei and micro-organisms are left. On treating pus in this way I found that a large quantity of granular matter remained, and, though I very soon arrived at the conclusion that *bacteria—i.e.* rod-shaped organisms—are not present in the discharge from cases treated aseptically, I could not say whether among the granular matter seen there were or were not *micrococci*. This difficulty is the greater as there is more granular matter in aseptic wounds than in others.

I therefore soon commenced a series of cultivation experiments. The following was the principle on which I acted. On introducing a particular form of organism into a suitable pabulum with precautions against the entrance of others, this form of organism will grow there. This being the case, theoretically one would only require to inoculate some suitable pabulum with various discharges—on the one hand to get a development of organisms, on the other to find the fluid remain free from organisms, and unchanged. Various preliminary experiments, which I need not detail, established this. For the present investigation some suitable pabulum must be taken, sterilised, and inoculated under proper precautions with discharges from wounds. If we have a really pure pabulum, and the inoculation has been carried out in such a way as to prevent the entrance of any extraneous organisms, the inference, where development occurs, would naturally be that organisms have

been present in the fluid from which the inoculation was made. If, on the other hand, the same method has been employed, and no organisms develop, the inference would be that no organisms existed in the fluid.

I first used milk, but for various reasons I gave it up, and tried Pasteur's and Cohn's fluids, and, after reading Schüller's paper, Bergmann's; but I found these artificial solutions too insensitive to be of any value for my purpose. I then used vegetable infusions, more especially turnip, and ultimately infusion of cucumber, which last seems to be very sensitive. I also employed in many cases an infusion f meat.

The infusion having been prepared, is filtered, introduced by syphon into Mr. Lister's double-necked flasks, boiled for twenty minutes, kept for some days (at least two) in an incubator, and then decanted under a spray of carbolic acid into smaller purified flasks, which are likewise placed in an incubator for several days before being used. These flasks are covered with cotton wool caps purified by heat or carbolic acid, or they stand on a glass plate and are covered by a glass cap and a glass shade, as described before in the case of Mr. Lister's liqueur glasses.

For the purpose of inoculation, small capillary tubes, such as those used for vaccination, were employed. These possess the advantage over needles, in that, while they take up a larger quantity of the discharge, they protect it from the carbolic acid of the spray during the transit from the wound to the flask. The tubes are dropped into the flask containing the cucumber, and this is then placed in an incubator kept at the temperature of the human body. (See Fig. 72.)

The procedure may be shortly described as follows:—The outer portion of the dressing having been removed under the carbolic acid spray, a tube which has been previously purified in carbolic lotion is heated in the flame of a spirit lamp in the spray, so as to drive off all the carbolic lotion and to render it dry. This tube is now rapidly introduced into the drainage tube, and from thence immediately into the flask which is opened in the spray close to the wound. The flask is then placed in an incubator kept constantly at a temperature of 98° Fahr. In the case where flasks with cotton caps are used

it is well in performing an experiment to wet the margin of the cap with carbolic lotion before lifting it, so as to prevent any dust from falling from the cap into the fluid. This is a very important precaution.

Having ascertained that the method proposed was perfectly trustworthy, I proceeded to the investigation.

In performing the experiments I always inoculated two flasks, and often another was taken and the whole process gone through in the same place, with this difference, that the tube in the latter case, when heated, was put directly into the flask without touching the wound. These latter flasks remained, without exception, clear.

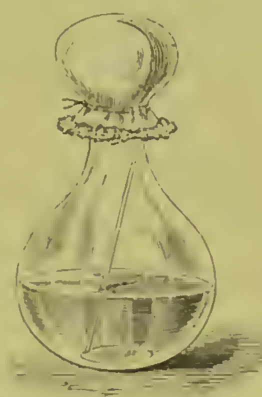


FIG. 72. FLASK CONTAINING CULTIVATING FLUID INOCULATED FROM A WOUND.

When development occurs in the flasks inoculated the fluid generally becomes muddy in 30 to 50 hours, but where the fluids remained clear I have kept them in the incubator for weeks, and then tested them by the addition of some substance containing bacteria.

As a result, I find that in cases treated aseptically, where of course there was an unbroken skin to start with, one of two things may happen—either the fluid remains *perfectly clear*, without the development of organisms, showing that none were present in the wound; or the fluid *becomes turbid* from the presence in it of organisms of the form seen in Fig. 1, Plate I. In both cases the wound may follow an aseptic course; *i.e.* no local or constitutional disturbance results from the operation, and from the appearance of the wound one could not tell in many cases whether these organisms were present or absent.

From Fig. 1 it will be seen that these organisms are minute spherical bodies arranged in pairs; in triplets, in which case they form a triangle (a very important point in distinguishing them from other forms); in groups of four (positions which bacteria never take up); also in short chains and groups of larger or smaller size. In fact they belong to the group of the schizomycetes termed *micrococci*.

I have said that in many cases one could not tell from the

course of the wound whether these organisms are present or absent, but sometimes their presence can be suspected. Those who have worked long at aseptic surgery will have met with cases where when a dressing is left on for six or seven days, or when a deep dressing is left for some weeks, the discharge acquires a sour odour and the skin around the wound becomes somewhat excoriated. As the wound in other respects follows an aseptic course, Mr. Lister concluded that this was probably a chemical change taking place between the discharge and the materials in the gauze dressing. Knowing the peculiar property possessed by salicylic acid of preventing chemical fermentations, Mr. Lister uses it in such cases with the effect of diminishing or preventing this change.

In these cases I have always found micrococci.

If micrococci be grown in a small quantity (3 to 8 drachms) of cucumber fluid, after three days they seem to die; at any rate, they will not grow in any liquid. But yet if the fluid be kept for some weeks it will gradually become red, till it ultimately is of a dark vermilion tint. Thus chemical changes continue after the activity of the organism has ceased. May not something of the same kind occur in these cases? Chemical changes are primarily set agoing by these organisms, but continue of themselves, and thus salicylic acid acts by preventing these changes, as Mr. Lister supposed, though, according to this view, the organisms are necessary for their commencement.

If now we contrast these results with those obtained in wounds not treated strictly aseptically we find this marked difference, that in *none* of the latter were *organisms absent*, while in almost all *bacteria as well as micrococci* were present. It is to be observed that in many of the cases antiseptics were employed, both in the external dressings and injected into the wound, but no precautions were taken either to penetrate to all the recesses of the wound with the antiseptic so injected, or to prevent the access of organisms during and after the dressings.

I may mention that in four cases which were originally treated aseptically *bacteria* were found, but in all these their presence was indicated by disagreeable smell or by symptoms of local or constitutional disturbance. It is thus evident that bacteria as well as micrococci can flourish under an antiseptic

dressing. The explanation of their absence must therefore be that the circumstances which permit of the entrance of micrococci are not such as to allow the advent of bacteria.

It was thus satisfactorily established that there was a very marked difference between the discharges of aseptic wounds and of those not treated aseptically. From the former, organisms were generally *absent* till about the end of the case when the dressings were left on for several days. In the latter, organisms are *present*, even within the first twenty-four hours. Again, in the former, when organisms did appear they constantly belonged to the group of micrococci, in the latter rod-shaped organisms were frequently present as well, and generally in large quantities if there was any putridity in the wound.

It was just possible that an objection could be brought against these results to the effect that organisms might have been present in the discharge of aseptie wounds, but that they were unable to develop in the fluid used for cultivation. To obviate this objection as far as possible I used a variety of cultivating fluids and got the same results with all.

During the spring and summer of 1880 I renewed the study of this subject in a different manner. I adopted Koch's method of staining bacteria [1] and I employed it in all Mr. Lister's cases from the beginning of March till the end of June (four months), and my results confirm in every respect those which I had got by the method of cultivation.

I find that, in the first few days after an aseptic operation no organisms can be found in the discharge, and that, when they ultimately do appear, they are micrococci, not bacteria. On the other hand, after operations not performed aseptically organisms are generally present from the first, and as a rule these consist of bacteria as well as micrococci.

The principle of this method of staining is that various aniline dyes, more especially methyl violet, fuchsin and aniline brown, stain chiefly the nuclei of cells and bacteria; though these are generally the only bodies stained, yet in some cases, especially if the staining is excessive, other albuminous granular matter may also become coloured. However, even where such is the case

[1] See Cohn's *Beiträge zur Biologie der Pflanzen.*

the organisms can, as a rule, be easily recognised by their form and arrangement. The pus or other fluid to be examined is spread in a very thin layer on a cover glass or slide, and left to itself to dry or dried over a spirit lamp. In the case of albuminous fluids it is well to do nothing more for at least twenty-four hours. These cover glasses may be kept for months and then used, for no organisms can grow on the dried materials.

In order to stain the specimens a few drops of a saturated solution of methyl violet or of fuchsin in alcohol are added to distilled water till a sufficient depth of colour is obtained. This can only be determined by experience, but it is well to stop before any precipitation can be detected. (Dr. Ogston recommends a watery solution of methyl violet of the strength of half a grain to the ounce.) A drop of this solution is allowed to flow over the cover glass, being retained in contact with the material to be stained for about one minute. It is then washed off with distilled water and the cover glass again dried as before. When quite dry it is mounted in Canada balsam.

When aniline brown is used a concentrated solution in glycerine is prepared. This is filtered, and one part of the filtrate is added to an equal quantity of distilled water and an equal quantity of pure glycerine. This mixture is now filtered and is then ready for use. I find that this fluid, while it does excellently for staining organisms in such fluids as cucumber, turnip infusions, &c., does not stain them well in pus. I find it best in the latter case to place a drop of the staining fluid on a slide, then lay on the cover glass, the material to be stained being of course lowermost. Leave this for twenty-four hours and then suck out the staining fluid with filter paper, introducing in its stead pure glycerine. This is a difficult process, and the specimens are frequently not quite clean. However, if one examines the layer attached to the cover glass, one sees what was in the material; the fragments which are floating free may consist of all sorts of débris. The specimen is then surrounded with cement.

The results of these methods of staining are very beautiful. If the staining is not too intense, only the nuclei of the pus-cells and any organisms which are present are stained, and the latter can be recognised with the greatest readiness with a sufficiently high power.

So much for the method. Plates I. to IV. illustrate the results.

Let us take first some specimens from wounds which have not been treated aseptically. Here it will be seen that there are always organisms, and that these generally consist both of bactèria and micrococci, though sometimes of one or other alone. Look at any wound not treated aseptically, which has not united by first intention, and which has been somewhat recently made, and you will get this result.

Case 1.—Fig. 2 is a specimen of the discharge taken from a compound dislocation of the thumb a few days after the accident. The wound had not been treated aseptically, and it had a very foul smell. (The patient, by the way, died of tetanus.) Here a great variety of organisms will be found—bacteria, bacilli of various kinds, and micrococci.

Case 2.—Fig. 3 is a specimen of discharge taken from a wound of the scrotum in which a small slough was lying. The wound was syringed out daily with carbolic lotion 1–40, and dressed with boracic ointment. The discharge had a very foul smell. Here there are multitudes of minute bacteria, bacilli, and micrococci.

Case 3.—Fig. 4 was taken from a case in the out-patient room, not treated aseptically. There was not much discharge and no putrid odour; rather a slightly rancid smell. Here well-marked bacilli can be seen. Discharge taken on two occasions presented the same appearance.

Case 4.—Fig. 5 was taken from a case of amputation of the thigh which had been done two days previously, and had been treated by irrigation, though I must say, for the credit of irrigation, not very efficiently. Here there was a slight smell. Bacilli are present.

Case 5.—Fig. 6 was taken from a case of excision of the hip-joint where numerous sinuses existed previous to the operation, and where, therefore, there was no hope of eradicating putrefaction. I dressed this case myself, washing out all the sinuses daily with 1–40 carbolic acid lotion, and applying boracic or salicylic acid ointment, and outside this boracic lint. The specimen figured was taken more than four months after the operation, and contains numerous bacteria; this, observe, although the wound had been treated assiduously for months with antiseptics, but not aseptically.

At the end of March some pieces of dead bone were felt, and these were removed on April 1st. The wound and sinuses were thoroughly washed out with chloride of zinc and dressed as before.

The discharge, taken two days later (on April 3rd), had a putrid odour, and contained numerous large and small bacteria.

On the following day (April 4th) it presented the same appearance. Sixteen days after this second operation (on April 17th) there were still numerous bacteria present

Case 6.—Take again a case of Syme's amputation performed on March 9th, 1880, for disease of the ankle-joint. Several sinuses were included in the flaps. Free drainage was employed, and salicylic or boracic ointments and boracic lint. This case went on very well; *i.e.* there was at first a little odour, but this was very slight and soon almost entirely disappeared. The discharge got less, and the wound came to look somewhat like an aseptic wound.

Fig. 7 (March 15th, from drainage tube) shows chiefly streptococci in pairs; no typical micrococci, nor the colony form of micrococci; small bacteria; a few long rods.

On March 17th the discharge taken from the drainage tube contained chiefly oval bacteria; also streptococci (*i.e.* spherical bodies in chains); no typical micrococci; a few long bacteria.

On April 12th the organisms were much less numerous, there being only a few streptococci and bacteria.

April 14th, very few organisms—streptococci.

April 16th, rather more organisms than in the last—only streptococci. There had been a little retention of the discharge.

May 14th, a considerable number of streptococci; also a few oval bacteria: drainage not quite perfect.

Here we see that organisms were present, but so long as the discharge flowed freely away, they did not develop. When tension occurred they grew. The wound did very well, and forms of micrococci were the chief organisms present.

So much for examples of cases not treated aseptically. In all cases organisms were present, and these were almost always *bacteria*. The more putrid the discharge, the more numerous and the smaller were the bacteria (Bacterium termo?). The better the progress of the wound, and the better the drainage, the fewer the organisms; but nevertheless there was always some form of organism present, and had I inoculated infusions from them, I should certainly, according to my former extensive experience, have got bacteria, as well as micrococci, to develop in all cases. The significance of the diminution in number of the bacteria in the last case is a point which I shall not discuss here. I will merely state that in some wounds following a

very satisfactory course *micrococci* only can be found by this method of examination.[1]

Let us now look at cases treated aseptically. I shall only mention a few instances, but I may state that I have examined, in almost all cases, specimens of discharge taken at every dressing with the same results as are illustrated here.

In the first place, I will give two cases which show that all forms of organisms may be permanently excluded by strict aseptic treatment.

Case 7.—Take first the most testing case of all—one of empyema. Here at each change of the dressings air is sucked in with every inspiration, but when the dressing is done with aseptic precautions, this air has been acted on by carbolic acid. We shall therefore see whether the spray is efficient in destroying organisms.

The case of empyema to which I refer was one of considerable standing, and was opened aseptically on March 7, 1880. The cavity of pleura was not washed out, and during the whole of the treatment no carbolic acid or other antiseptic was applied to the interior. Hence if organisms got in, they could develop just as freely as in a flask. There were no organisms in the pus when evacuated. The dressing was changed daily.

Fig. 8 is a specimen taken on March 15th, eight days after the thorax was opened. This contains no organisms of any kind, neither bacteria nor micrococci. Hence for eight days the spray had been efficient.

Fig. 9 was taken on April 13th, *i.e.* thirty-seven days after the incision. Specimens examined in the interval were free from organisms, and here it will be seen that there are no organisms of any kind. This result is the more convincing, as for some days there had been a difficulty to the exit of the fluid, and some discharge was pent up in the lower part of the thorax. This was let out on April 13, and the specimen was taken from this fluid. Now my invariable experience has been, that when such accumulation occurs, if organisms were present before, they will be found in large numbers in the retained fluid; in fact they develop just as freely as if the fluid were in a flask. A specimen taken on April 19th—*i.e.* forty-three days after the incision—was also free from organisms.

This case seems to me an absolute proof of the efficacy of

[1] In these cases, though micrococci alone could be found, yet cultivation experiments would in all probability have revealed the presence of bacteria as well.

the spray in destroying the activity of the particles in the air which give rise to organisms and fermentations; for here the action of the living tissues, to be afterwards discussed, could not come into play—the fluid was under the same conditions as if it had been in a flask placed in an incubator. The conditions exactly correspond with my spray experiments (p. 26 *et seq.*).

Case 8.—Take next a case of incision into the knee-joint in a case of gelatinous degeneration of the synovial membrane before suppuration had occurred. Mr. Lister has found that, in these cases, free incisions on each side of the patella, and the insertion of a drainage tube into the joint, often brings about a cure without suppuration and without the necessity for further operation. It was so in this instance. The incision into the knee was made, and drainage tubes inserted, on May 10th.

On May 14th *no organisms* were present in discharge taken from the drainage tube.

On May 16th, same result. Fig. 10 is a specimen taken on May 20th from the drainage tube: *no organisms.*

A specimen was also taken on May 20th from a plug of lymph in a small chronic abscess beside the knee, opened on May 12th, and here also there were no organisms.

On May 22nd there were still no organisms.

I might mention a number of cases to show that where the dressing is frequently changed, organisms may remain absent for a long time, or even altogether. Therefore where the dressing is changed frequently, and where the various aseptic precautions are thoroughly carried out, organisms never develop in the discharges.

As a rule, however, the dressing is not changed so frequently, and then, though organisms are absent at the commencement of the case, they frequently appear towards its termination. In that case, however, the organisms which appear belong to the group of micrococci.

The following cases illustrate this.

Case 9.—I will take first a case treated strictly aseptically, and following a typical 'aseptic course.' This case illustrates the entrance of micrococci after some days.

The patient, a young man æt. 26, had suffered for a long time from a sore on his leg, which had now become epitheliomatous. The

patient was very weak. Amputation was performed through the middle of the thigh. The case followed a typical course. There was no rise of temperature. The patient felt at once relieved by the removal of the disease, and his appetite and strength began to improve from the day of the operation. The wound healed by first intention, except where the drainage tube was. When this was removed the sinus became filled with lymph, and this becoming vascularised, healing took place completely without the occurrence of granulation. The amputation was performed on April 8th.

Fig. 11 was taken on April 9th (first dressing) from the drainage tube. *No organisms*, but there is a good deal of granular matter.

Fig. 12, taken on April 10th from the drainage tube (second dressing). *No organisms.* Less granular matter. Much less discharge. The dressing was now left unchanged for two days.

Fig. 13, taken on April 12th from the drainage tube. *No organisms.*

The dressing was again changed on April 14th, and one or two bodies were then seen which might be micrococci, but of this I could not be certain.

Fig. 14, taken April 16th. A piece of lymph was clipped away and rubbed over the surface of a cover glass. There was almost no discharge. Line of incision soundly healed. The piece of lymph filled up the place where the drainage tube was. (The drainage tube was removed on April 14th.) *Distinct micrococci*; no bacteria; almost no leucocytes—those that are present being badly formed. The dressing was now left on for three days, and in a specimen taken on April 19th from the little bit of lymph there was *nothing but micrococci.* No bacteria and no leucocytes. The dressing was now left on for four days.

In another specimen taken on April 23rd, also from the lymph, which had by this time become in the main vascularised, *micrococci* were present in great numbers. No bacteria.

Here we see the typical result in a case where the ordinary rule was followed of not changing the dressing till the discharge comes to its edge. In this case displacement of the dressing was the cause of the frequent changing of the dressing latterly. For six or eight days no organisms appeared in the discharge. When they did appear they were micrococci. Bacteria never got in. Further, the micrococci, though latterly present in enormous numbers, never caused suppuration, nor did they apparently interfere with the healing of the wound.

Case 10.—Case of excision of the mamma and axillary glands for scirrhus, done on March 19th.

Fig. 15, taken from the drainage tube on March 20th, contained *no organisms.*

A specimen taken from the drainage tube on March 21st contained *no organisms.*

A specimen taken from the drainage tube on March 22nd contained *one or two micrococci.*

These micrococci were more numerous at the next dressing on March 26th, and they were present in large numbers on March 31st and on April 2nd; no bacteria having appeared, as will be seen in the next specimen.

Fig. 16, taken on April 4th, when the wound was almost completely healed. Here there were *numerous micrococci*, but no bacteria.

In this case the micrococci got in earlier than we have yet seen—viz. on the third day after after the operation — but nevertheless bacteria never appeared, and the wound did not seem any the worse for the presence of the micrococci.

That micrococci may get in even earlier than this, if there is but little overlapping and much discharge, is evident from a case of removal of a small epithelioma from the cheek, where only a small dressing was applied, which was left on for two days. The discharge obtained on the second day—*i.e.* at the first dressing—was found to be full of micrococci. The edges of the wound were not brought together, but it became filled with blood-clot, and healing occurred under this without any suppuration at all, and more rapidly than I have ever known it take place in such a wound, and yet numerous micrococci were present even from the first.

Case 11 also illustrates this. A keloid was removed from the back of a man's neck on March 24th. A small dressing was applied, but there was a good deal of discharge, which reached the edge of the dressing a few hours after the operation. Specimen 17, taken at the first dressing on March 25th, shows *a few micrococci.*

A specimen taken on March 28th (third dressing), and one taken on March 31st, showed the presence of micrococci in large numbers, but no bacteria appeared at any time.

And now I come to two cases illustrating very important points as to the source of these organisms. In these cases I

have examined the discharge, not merely in the drainage tube, but at some distance from the wound, under the gauze dressing, and I have found that, though there might be no organisms in the wound, yet they might be present at the edge of the dressing, and that micrococci had generally advanced nearer to the wound than bacteria.

Case 12.—Excision of the mamma and axillary glands, done on March 27th.

A specimen taken on March 28th from the drainage tube (first dressing) contained *no organisms.*

Discharge was taken on March 31st (third dressing) from the dressing, *at a considerable distance from the wound.* The dressing had not been changed for two days, and the discharge had reached the edge some hours before the visit. *This specimen contained both micrococci and bacteria.*

A specimen of the discharge taken at the same time from the *drainage tube contained no organisms.* (See Fig. 18, Plate III.)

Examined again on April 2nd (from drainage tube, fourth dressing). *No organisms found.*

Discharge taken on April 4th from the *dressing*, at some distance from the wound, contains *bacteria and micrococci.* (See Fig. 19, Plate III.)

Fig. 20 is from a specimen of the discharge taken at the same time from the drainage tube, and contains *no organisms.*

A specimen taken from the sinus on April 6th contained *no organisms.*

A specimen taken from the sinus on April 8th contained *a few micrococci.* Wound almost healed.

A specimen taken at the next dressing from a piece of lymph over the orifice of the sinus showed *numerous micrococci.*

Here we see that on March 31st organisms had penetrated for a little distance under the dressing, but had not yet reached the wound.

The same was found on April 4th ; but on April 8th a few micrococci had got in, and having once got in they multiplied rapidly. *Bacteria did not get in.* There was no change in the appearance of the wound to show that anything hurtful had entered.

Case 13.—This was a case of disease of the knee-joint treated like Case 7, but here there was necrosis of the patella before operating, and extensive disease of the bones. As no improvement followed the

incisions, excision was performed on April 2nd. The drainage tube on the *inner* side passed in between the bones, and one was also introduced into a hole gouged in the bone; that on the *outer* side passed into an abscess cavity in the soft parts. The case followed the typical course.

A specimen was taken on April 2nd from some curdy material found in the interior of the joint. (It must be remembered that the interior of the joint had been in communication with the outer world for some weeks by means of a drainage tube, but there had never been any suppuration in the cavity of the joint.) *No organisms* were found.

A specimen taken on April 3rd from the *drainage tube* on the inner side contained *no organisms.*

Fig. 21, taken on April 4th from the gauze at some distance from the wound on the inner side. *One or two micrococci; no bacteria.*

Fig. 22, taken on April 5th from the *inner drainage tube,* contained *no organisms.* Contrast this with Fig. 21, taken from the gauze on the previous day. In it there were a few micrococci at some distance from the wound, but, as we see from Fig. 22, they did not get in.

Fig. 23, taken on April 8th from the *inner drainage tube* contained a *few micrococci.* They had now reached the inner wound, and in later specimens they were found in large numbers.

Fig. 24 is a drawing of a specimen taken on April 15th from the *outer drainage tube* leading into the abscess cavity. This contained *no organisms,* although they were present on the inner side of the knee on this same day in large numbers. This shows that the organisms could not have come through the blood or developed spontaneously in the wound; otherwise they ought to have been found in the outer side as well as in the inner.

From these results—and these are only a sample of what I have got by this method of investigation—and from my former cultivation results (each method very important in its own way), the difference which I have been led to establish between wounds treated aseptically and those not so treated will be evident.

Wounds treated aseptically are either free from organisms or, if the latter are present, they are only micrococci. The others always contain organisms, and, in the great majority of cases, these organisms are bacteria as well as micrococci.

III. If, under any circumstances, organisms do enter wounds treated aseptically, what are their peculiarities?

The facts just stated under the second heading imply that we have to deal in wounds with two great groups of organisms—rod-shaped organisms, or bacteria; and spherical organisms, or micrococci.

Some, however, assert that there is no specific difference between micrococci and bacteria; and these observers would say that the micrococci found in aseptic wounds are simply bacteria altered in form by the new conditions in which they are placed. Prof. Billroth, indeed, has gone so far as to assert that there is only one species, coccos, in the group of Schizomycetes; that this may under varying circumstances assume the form of bacterium or coccos, these two being transmutable into each other. That micrococcus is an organism distinct from bacterium, is denied by Hallier and doubted by Klebs, while it is strongly affirmed by Cohn, Rindfleisch and others.

I am now thoroughly satisfied that micrococci are really a class of organisms quite distinct from bacteria. I have observed them and worked with them for four years, and I have never yet met with an instance in which a micrococcus has become a bacterium, or *vice versâ*.

Before considering the evidence on this point, I may define what I mean by micrococci. They are (following Cohn) colourless or coloured round cells, very small, generally under one micro-millimetre in size, with or without movement, growing in pairs, triplets arranged in triangular form, short chains or groups of smaller or larger size, not derived from bacteria nor developing into them. Other living spherical bodies may be found in cultivating fluids, such as spores of fungi or, indeed, of some forms of bacteria, as pointed out by Koch [1] and Ewart [2]; these, however, when fresh nutriment is added, develop again into fungi on the one hand, and into bacteria on the other. The life history of micrococcus seems only to consist in development from pairs to short chains or groups of larger or smaller size, this cycle being repeated on the addition of fresh pabulum.

I need not here enter into the general characteristics of these organisms, but I may mention some facts which tend to shew that they are distinct from bacteria.

[1] *Beiträge zur Biologie der Pflanzen*, Bd. ii., 1876.
[2] *Microscopical Journal*, vol. xviii.

Tiegel[1] has pointed out, that if a fluid containing organisms be made strongly alkaline with carbonate of soda, *bacteria* quickly disappear and only a few *micrococci* remain, which also ultimately vanish. This indicates a chemical difference between the two forms.

A similar chemical difference has been incidentally referred to by Koch[2] viz., that while *micrococci* are stained by hæmatoxylin, *bacteria* are not.

I have found that micrococci, when acted on by a strongly peptic solution at the temperature of the human body, remain unaffected, in contrast to ordinary albuminous granular matter, which, as a rule, soon disappears. Many forms of *bacteria*, similarly treated, become aggregated into clumps, or may for the most part disappear, only a few irregular rods and granules remaining. Here again a chemical difference is evident between the two groups of organisms.

Then as to their mode of growth. I have mentioned the triangular arrangement of micrococci, and said that this is typical of these organisms. This is supposed by Cohn to be due to a looseness of the intercellular substance, allowing the cells to become displaced. But then this arrangement is constant, and Mr. Lister[3] has by direct observation made out that it is due to longitudinal division of the cells (Fig. 25). This is a method of division which never occurs in *bacteria*. Here again, then, in their mode of growth we have a marked difference between the two.

Micrococci prefer acid fluids; most bacteria prefer alkaline or neutral fluids.

Micrococci grow readily, as we shall presently see, in fluids containing proportions of carbolic acid in which bacteria only grow with difficulty.

Then, lastly, direct observation has failed to show any transformation of one into the other. I have specimens of micrococci which have remained in cucumber infusion for ten months, and they were just as perfect micrococci at the end of the time as at the beginning. There was no transformation into bacteria.

[1] Virchow's *Archiv*. Bd. lx.

[2] Traumatic infective diseases.

[3] *Transactions of the Royal Society of Edinburgh*, vol. xxvii. 1875.

I figure three specimens to illustrate the same point, and in the note on the next page I give a table of the series of experiments.

The micrococci were obtained from a case of acute abscess in the groin. When this abscess was opened some of the pus was introduced into flasks containing meat and cucumber infusions, but nothing developed. On the third day after it was opened, the dressing having been left on for two days, another flask of meat infusion was inoculated, and in this micrococci developed. From this flask were obtained the micrococci in the series of experiments tabulated below. They were cultivated for several weeks in a variety of fluids, but always remained micrococci of the same kind and with the same effects on the various fluids.

Fig. 26 represents the discharge from the wound from which the micrococci were obtained, and is seen to contain micrococci alone.

Fig. 27 is a drawing of a specimen taken from a flask of vitreous humour, the third in the series, and here we find only micrococci.

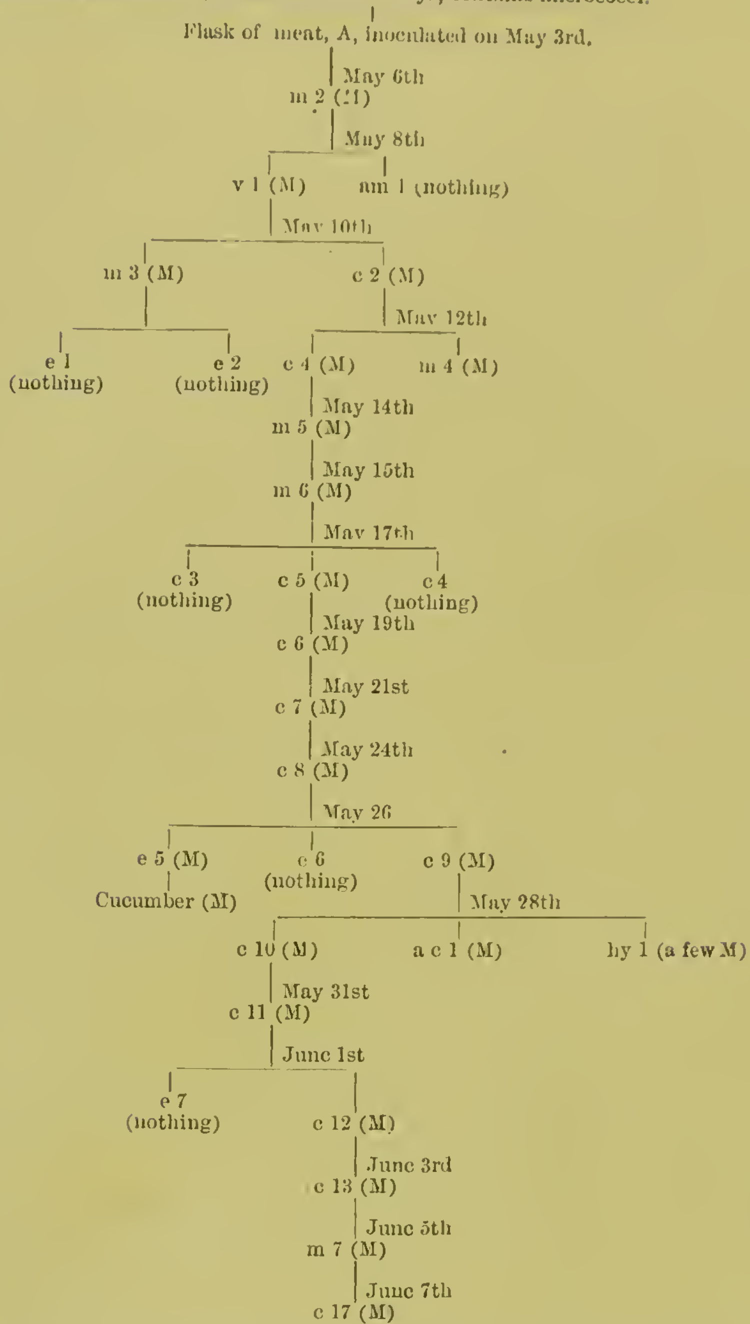
Fig 28 is taken quite at the end of the series (from c 17, see note), after the organisms have grown in a variety of fluids (eighteen in all), and yet here we have only micrococci, and these are similar in appearance to the others. There was here no development of micrococci into anything else. (I may just call attention in passing to the proof of the efficiency of the method of experimentation afforded by these results, and also to the numerous arguments which can be drawn from them in favour of the views I am advocating.)

Such are some of the facts which seem to me to show that micrococci are distinct from bacteria, and they are a sufficient answer to the question as to whether there is anything special about the organisms found in wounds treated aseptically.

It is only one well-defined class of organisms which enter wounds treated aseptically, while in other wounds all forms may be found.

I will not here enter on the question of the effects of these micrococci nor will I stop to enquire why it is that no bad

May 8th, abscess in groin, dressed after two days, contains micrococci.



Lost from having allowed four days to elapse before inoculating again.

In this list m = meat } infusion.
,, c = cucumber }
,, v = vitreous humour of sheep.
,, e = fresh egg.
,, am = alkalised meat infusion.
,, hy = hydrocele fluid (very concentrated).
,, M = micrococci present.

effects result from their presence. I shall content myself just now with the following remarks.

It is certain that they do not cause putrefaction, but they always cause a sort of sour, sweaty smell in fluids—a smell which can be recognised in whatever fluid they grow: in other words, they are associated with a peculiar fermentation. Now, the products of this fermentation are but little irritating. They have no acrid taste, nor do they feel pungent when applied to a cut surface. Hence, probably, it is that we find that wounds in which these organisms exist, even in large numbers, appear often unaffected by their presence.

Nevertheless, they can hardly, under any circumstances, be indifferent, and I think I have observed that, in some cases, after they have got in, the wounds do not behave quite so typically as usual; *i.e.* there may be a trace of suppuration, or a sinus takes longer to heal than one had any reason to expect. Again, if they get into a wound containing a piece of dead bone—say, not yet loose—they will grow in its canals, produce their sour products, and irritate the parts in the vicinity; and thus the bone, though not mechanically irritating, because not loose, nor yet chemically irritating if it is quite aseptic, does become somewhat irritating and loses its character of an innocuous dead piece which may be slowly removed by absorption by the neighbouring tissues, and becomes a foreign body which must be thrown off. In such cases, then, it would be of great importance to exclude these micrococci if possible.

An instance which, I believe, illustrates this, occurred recently in Mr. Lister's practice. We know that formerly, when hempen ligatures were used, they always came away. If, however, they are applied aseptically, they remain without causing suppuration, as has been shown in Mr. Lister's cases. The wound heals over them, and they may indeed ultimately disappear. But here, as in the case of the dead bone, a necessary condition for obtaining this result is, that the ligatures be perfectly unirritating. In the case I refer to, Mr. Lister excised the thyroid gland aseptically, having previously ligatured the vessels, somewhat after Watson's method, with strong hempen twine. The wound healed entirely by first intention, except where the drain was, and this also had almost healed by the tenth day. For

some days previously, however, the dressing had been left unchanged, and about the tenth day the discharge, though still remaining in the main serous, increased in amount. This state of matters continued till one by one the ligatures came away. The ligatnres, when examined, were found to have a distinctly sourish 'micrococcal' smell and an acid reaction, and under the microscope their interstices were seen to be filled with micrococci. Here we have a perfectly aseptic course for the first few days till the time came when the dressings were left unchanged for several days—till, in fact, as we have seen, micrococci got in. When these organisms appeared they grew in the interstices of the thread, produced their acrid products, and the thread was no longer a substance which might become encapsuled or even disappear by absorption, but it became an irritating foreign body, which had to be thrown off before healing could occur.

In the same manner, if micrococci entered a healthy joint where we have a cavity containing nutritious fluid, rather in the conditions of fluid in a flask than in a cavity in the living body, I would hardly regard their presence with satisfaction; for I should fear that their products would not be altogether neutral to the sensitive synovial membrane, and might, at the very least, cause increased secretion and delay in healing.

These facts are of themselves sufficient to indicate the advisability of taking all possible precautions to exclude these organisms. Dr. Ogston has recently assigned much more serious consequences to them, but the subject is at present too debateable to be suitable for discussion here.

IV. Are organisms present or do fermentations occur in fluids or tissues in the living body which have never been exposed to atmospheric dust? If they do so occur, how is their presence to be explained?

This question especially deals with the occurrence or not of organisms in the healthy living body. I need not enter at length into this question again; I have twice already touched on the subject. I have described the experiments by which it has been shown that the blood, urine, and milk of healthy living animals contain no organisms, and possess no inherent tendency to undergo fermentative changes. Then I narrated,

both at p. 45 and at p. 196, my experiments on the *tissues* of healthy living animals. I need not dilate further on this question, but the conclusions to which we were forced were, that the blood and tissues of healthy living animals do not contain organisms or their spores, and have no inherent tendency to undergo fermentation. But it may be objected that these organs were removed from the living body and placed in flasks, and that the conditions were not the same as if these tissues had been retained in relation with the living body. Such an objection is of no value, because any difference in the conditions is in favour of organisms in the flask experiments, for the walls of the flask are neutral to their development, while healthy living tissues are powerful destroyers of bacteria. This we shall see presently. In the meantime, I will now bring forward a piece of evidence which completely sets any such objection at rest. I refer to what is known as the 'expérience du bistournage.'[1]

This 'bistournage' consists in rupturing the spermatic cord subcutaneously by torsion. The testicle is thus separated from its nutritious vessels, and lies loose in its tunics which protect it from the access of the air. It adheres to the tunica vaginalis, but the new circulation is insufficient; the testicle atrophies and disappears. There are no accidents, because, as we have previously shown and shall immediately see, the air with its dust—*i.e.* bacteria—does not get access to the dead part.

M. Chauveau who is the author of these experiments, and whose name is sufficient guarantee for their scientific accuracy, proved this in the following way:—If the harmlessness of the operation depends on the absence of the putrefactive organisms which would be carried to the testicle by the air, then these accidents ought to occur when the germs are carried to it by the blood. Chauveau accordingly injected organisms into the vascular system of rams. After the fever, sometimes fatal, caused by this injection had subsided, he performed the operation of 'bistournage.' Putrefaction of the testicle occurred in those animals which had been injected with bacterial liquid.

[1] See Jeannel's book, *De l'Infection Purulente*, from which the facts are taken.

Several objections could be urged against the experiments, and are answered by Chauveau :—[1]

'1. Nothing proves that the bacteria in the fluid injected were the active agents. But the experiment repeated after careful filtration of the fluid by special filters—*i.e.* after removal of bacteria or their germs, remained without effect.

'2. Nothing proves that it was not the infective fever itself which caused the putrefaction of the organ. But of two rams injected with the same fluid and the same dose, putrefaction only occurred in the case where 'bistournage' had been practised. Still further, if in the same animal 'bistournage' is practised on the *left* testicle *before injection*, and *on the right after injection*, the *right* testicle is the only one which putrefies: a proof, evident and very ingenious, that it is indeed the penetration of the putrefactive germs into the organ which determines putrefaction; since the testicle which was separated from the general circulation before the injection remained indifferent, and did not undergo putrefaction, in spite of the infection of the whole body.'

I have also referred to several similar well-known facts at p. 50 *et seq.*, and Dr. Ogston has tested such fluids as extravasations of blood, hæmatoma, contents of pathological cysts, the fluids of the natural cavities of the body, &c., by means of Koch's method of staining, and he has failed to find any organisms. I have also referred to Meissner's recent facts.

From all these considerations we may, I think, conclude that organisms are not present in the fluids or tissues of the *healthy* living body.

Are organisms present in the body in states of disease? (I leave out of account here the infective diseases.) If so, how is their presence to be explained?

If we investigate animals suffering from disease, we shall find that in certain cases organisms are present. What these exact conditions are I am not yet able to say, but I will indicate some of the cases.

I have found that if an acute inflammatory process be induced in an animal—say by the injection of ammonia—subcutaneously, as pointed out some years ago by Dr. Burdon-

[1] Quoted from Jeannel.

Sanderson, the organs examined by the method I have described may be found to contain organisms.

I have in three instances produced abscesses in rabbits by the injection of croton oil, and in one case I have found that the pus of the abscess contained micrococci and bacteria, though none were introduced at the time. In one of the others I found only a very few bacilli and in the third I found no organisms. I shall detail the experiments.

Experiment 1, March 24th, 1881.—Pure croton oil was introduced into a tube. This was then sealed at both ends, placed in a sand bath, which was raised to a high temperature (about 270° F.), and maintained at that temperature for about 2 hours. When it had cooled, one end of the tube was carefully opened in a spray of carbolic acid, and a pure syringe being rapidly introduced, some of the oil was sucked up, and at once, in the same spray, half a minim was injected into the dorsal muscles of a mouse. The skin of the mouse was very carefully purified beforehand, and care was taken not to allow any of the oil to escape along the needle track either in introducing or withdrawing the needle. The puncture was dried with a carbolised rag and then touched with collodion.

A flask of meat infusion was inoculated from the oil at the same time.

On March 25th (26 hours later) the animal was found dying, and was at once killed. Great care was taken, in removing the skin from the back, to avoid contamination of the deeper parts. The muscles at the seat of injection were found to be infiltrated with pus, but no trace of the needle track through the skin could be seen. On examining this pus after straining, it was found to contain large numbers of organisms, chiefly micrococci, but a few bacteria and bacilli could be seen.

The meat infusion remained permanently pure.

Experiment 2, April 12th, 1881.—A mixture of equal parts of croton oil and olive oil was purified and 3 minims injected into the dorsal muscles of a large rabbit with the same precautions as in Experiment 1. A flask of meat infusion was inoculated with this mixture at the same time but nothing developed in it.

On April 18th the animal was observed to be ill, and it was

therefore killed. A considerable quantity of pus was found at the seat of injection. On examination of this pus I found numerous pus cells, but I have not been able to satisfy myself that there are any micrococci. A very few bacilli were present.

Experiment 3, April 20th, 1881.—2 minims of the croton oil mixture used in Experiment 2 were injected into the dorsal muscles of a large rabbit, with the usual precautions.

On April 25th, though the animal was perfectly well, I killed it. On examining the seat of injection I found one or two very small cheesy spots (small abscesses). These consisted of pus cells and granular matter, but I have not been able to detect organisms of any kind.

In each of the experiments the pus was stained with methyl violet, according to Koch's plan, and examined with Zeis's $\frac{1}{18}$th oil or $\frac{1}{25}$th water immersion objectives.

Again, I find that if the nutrition of an animal be profoundly interfered with, as in slow poisoning by phosphorus, organisms may after some time be found in the blood and tissues. Here, it may be said, inflammation of the intestinal tract has been caused, and the epithelial barrier against the entrance of organisms has been removed. But *the blood and tissues, when in a healthy state, have the power of themselves of destroying organisms* when these are introduced into the body. Thus, if into the veins of four medium-sized rabbits I introduce $\frac{1}{4}$, $\frac{1}{2}$, $\frac{3}{4}$, and 1 c.cm. respectively, of fluid containing bacteria, keep the animals alive for at least twenty-four hours, then kill them and preserve their organs in the manner described, I shall probably find no organisms in the first three, while in the last they may or may not be present. Where larger quantities of the fluid have been introduced, they will certainly be found. Thus, even though the organisms could gain access to the blood, yet so long as the nutrition of the animal is fairly well carried out, they would be destroyed.[1] The reason that they are found where large quantities of bacterial fluid are injected seems to me to be that, along with the bacteria, their products

[1] Similar facts have been made out by Traube and Gscheidlen. See a paper by these authors, 'Ueber Fäulniss und den Widerstand der lebenden Organismen gegen dieselbe,' *Berlin. Klin. Wochenschrift*, No. 37.

are introduced, that these act in the same way as phosphorus, as poisons, and that thus the resisting power of the animal is diminished.

The conclusion which I would draw from these facts is, that in severe inflammatory processes, or in great interference with the nutrition of an animal, organisms may pass into the blood without losing their vitality. The organisms usually found are micrococci.

I have examined a large number of abscesses, in man, when opened. At first I only used cultivating experiments, and this paragraph gives the result of these. In *chronic* abscesses I have not as yet found any organisms, and in this my results agree with those obtained by Billroth by microscopical examination alone. Of *acute* abscesses I had up till May 1879 inoculated from thirty-two cases. In twenty-five of these no growth of organisms occurred, while from six micrococci were obtained. In no case did I get bacteria (I omit here one abscess in the ischiorectal fossa, where I found both bacteria and micrococci; and one with fæcal odour in the lumbar region, from which I did not inoculate when opened, but in which undoubted bacteria were seen on microscopical examination).

Professor Billroth has paid special attention to this subject, and he has found organisms in a larger proportion of acute abscesses than I have done. Professor Billroth likewise only mentions the occurrence of micrococci. In acute osteomyelitis, where a communication had not yet been established with the external air, organisms have been found in the medullary canal or in the pus surrounding the bone by Von Recklinghausen,[1] Klebs,[2] Eberth,[3] and Godlee.[4] These organisms were as a rule *micrococci*; bacteria have but very rarely been present.

In 1880 Dr. Ogston,[5] of Aberdeen, published an elaborate and careful research on the relation of micrococci to abscesses. In examining pus and discharges for organisms he made use of Koch's method, before described. In the first place, he found, as I had done, that micrococci were not present in chronic

[1] *Deutsche Zeitschrift*, Bd. iv. p. 239.
[2] *Ueber Schusswunden*, 1871.
[3] Virchow's *Archiv*, Bd. lxvi p. 341.
[4] *Lancet*, November 21, 1874.
[5] Published in Langenbeck's *Archiv*, Bd. xxv. Heft 3.

abscesses. In acute abscesses, however, he states that these organisms are always present. This fact, obtained by staining the pus, differs, as will be seen, from my cultivation results, and agrees more with Billroth's results from simple microscopical examination.

Since this statement was made I have examined a number of abscesses by staining the pus obtained when they were opened, and I now quite agree with Dr. Ogston. Organisms are always absent from chronic abscesses. Micrococci are always present in acute abscesses.

Fig. 29 (Plate IV.) is a specimen taken from a chronic abscess over the sternum. It contains no organisms.

Figs. 30 and 31 are taken from acute abscesses—one of the mamma, and the other of the finger. These contain micrococci.

In one case of abscess in the neck, which had been forming for about three weeks, I could find no organisms. The skin was red over it, and I have entered it in my notes as an acute abscess. In another case, which I have also in my notes as an acute abscess, no organisms were found. This was a case of small abscess in the thigh, in a situation where diseased bone had formerly existed. The patient positively asserted that the abscess had only been forming for fourteen days, but it is of course quite possible that it had existed longer, and had only begun to point for fourteen days. Nevertheless the man's positive assertion—for I questioned him again after I saw that no organisms were present—the redness of the skin, the well-formed pus cells, and the absence of fatty débris make it difficult to assert that this was a chronic abscess.

Notwithstanding those two doubtful cases, I am ready to accept Ogston's statement and to receive it as a law, that if the pus taken from an acute abscess when opened be examined after staining, micrococci will always be found.

How, then, are my former results to be explained? By cultivation, from thirty-two cases of acute abscesses I only got micrococci in seven instances, and yet we now know that had I stained the pus I should in all probability have found micrococci in all.

Well, in my recent investigation I not only stained the pus, but tried cultivation experiments, and I got much the same

results as formerly: in some cases micrococci grew, in others I got nothing. It seems, as far as I can judge from my facts, that if the abscess is opened soon after its commencement micrococci almost always develop. If, however, ten days or a fortnight or more elapse, these organisms do not as a rule grow. Why is this? Simply, I believe, because the organisms have died.

I made out early in this investigation, and have often confirmed it since—sometimes, indeed, to my great inconvenience—that micrococci very soon exhaust the nutritive material in a fluid, and that they then fall to the bottom and die, for they will not grow in any fluid nor increase on the addition of fresh nutriment. In a flask containing say an ounce of cucumber or meat infusion, the micrococci grow rapidly, but they do not live more than three days. Then they fall to the bottom, and the fluid at the top becomes clear and remains so permanently. Take a drop from the flask during the first three days and put it into another specimen of pure cultivating liquid, and micrococci develop readily. After the third day or later, in proportion to the amount of the fluid, no development occurs whatever one does. The organisms are dead. And so in an abscess they live as long as they find nutriment, and then they die and cannot be obtained on attempting cultivation, though they may still be seen on microscopical examination.

Ogston relates similar facts, though he apparently does not attach this meaning to them. He remarks that when micrococci are cultivated in flasks containing such fluids as urine, ascitic, ovarian or hydrocele fluid, blood, &c., the fluid at the surface remains for months clear, while a slight deposit, which consists of micrococci, is present at the bottom. He concludes that the micrococci are growing away from the air, and that therefore they are anaerobes. This is not the case, for micrococci grow with greater rapidity and luxuriance in pure oxygen than under any other circumstances which I have yet tried. The truth, I believe, is, that these organisms were at first not quite dead, but had very little vigour and soon ceased to grow and died. They were not growing away from the air.

His results in cultivation from abscesses confirm mine, for he says: 'Often the micrococci grew luxuriantly, sometimes in

chains and sometimes in groups, but *oftener* the experiments were unsatisfactory, and it indeed happened that the micrococci which were introduced died altogether.' Now had Ogston put these two facts together, and had he worked for long with cultivation, he would, I venture to think, conclude as I do, that the micrococci which are found in abscesses, but which will not develop in the cultivating fluid, *are already dead in the abscess* and do not die only on removal from it.

The following fact may be mentioned as bearing out this view:—

Fig. 32 (Plate IV.) was taken from a case of acute abscess in the groin, which was opened on April 29th. A number of streptococci will be seen. Nevertheless, a flask of meat and one of cucumber inoculated at the same time remained quite barren. A specimen was taken from the same case at the next dressing, April 30th. There was plenty of discharge in the drainage tube, but this contained almost no organisms. Had the organisms been alive on the 29th, they would probably have been as numerous, if not more so, in the interior of the drainage tube on the 30th.

So far, then, I conclude, that though micrococci are always present in acute abscesses, yet if the abscesses be not opened for some time, these organisms will be found to have died. I shall not enter here into the question of the relation between these organisms and the abscesses in which they are found. The facts are sufficient for my present purpose.

It thus seems, that in certain states of low vitality and in acute inflammations, organisms may be present in the blood and tissues of animals. These organisms are generally micrococci.

V. How do organisms get into wounds treated aseptically? There are three possible explanations which might be offered:— 1. They come from the blood. 2. They arise spontaneously in the wound. 3. They come from without through some insufficiency in the aseptic method.

1. They come from the blood. We have already seen that organisms are not present in the blood or tissues of the healthy living animal, but that they may occur in low or inflammatory states. But a person on whom an operation has been performed

aseptically is, after the effects of the chloroform have passed off, and provided that there has been no great loss of blood, practically as well as before the operation. There is no inflammation and no febrile disturbance—the patient, as far as one can judge, ought to be as able to resist the entrance of organisms into his blood as before the operation.

If, however, these micrococci did enter the wound from within, they would do so during the first few days after the operation ; but if we look at the cases described, and the same thing can always be found, we see that these organisms do not, as a rule, enter for several days after an operation—not till the dressings are but infrequently changed.

In the case of acute abscesses opened early, and in which the micrococci are still alive, they of course exist from the first; and so they may occur if for any reason acute inflammation attacks a wound. But that this is their mode of entrance in ordinary cases is against all the evidence.

2. As to spontaneous generation. We have discussed that at such length at various places that I need not enter into it again. The facts with regard to the absence of organisms under certain circumstances and the constant presence of a particular form when they do occur, together with the points to be presently mentioned, sufficiently do away with any necessity for considering a view which can only be thought of where other and more natural modes of origin cannot be traced.

3. We must then search for some mode in which they might enter through the antiseptic arrangements. These we may divide into three parts : a lotion in which the various substances are soaked before being brought into contact with the wounds ; a spray to purify the atmosphere ; and a dressing so constituted as to give off carbolic acid to the discharge as it passes under it.

That the lotion is sufficiently potent to destroy organisms which come in contact with it, will be very evident to any one who chooses to examine the subject. If one places a preparation of actively moving bacteria under the microscope, and allows a little carbolic acid lotion, 1 in 20, to flow under the cover-glass, the movements will be seen instantly to cease ; this, in fact, is the method which I formerly employed when I

wished to draw moving bacteria with the aid of the camera lucida.[1]

That the spray is sufficient has already been shown by a number of experiments described at p. 26 *et seq.* (see also the case of empyema, p. 237).

An attempt has been lately made by Dr. Lewis Stimson [2] to show that the spray does not act as a germicide, and as his results have been extensively quoted, I must briefly notice them here. Three tubes were filled with urine, boiled, and allowed to cool in the spray and then exposed in it for an hour and a half, the floor being swept to raise the dust. The tubes were then closed with plugs of cotton-wool wet with alcohol. Particles of dust were seen to be caught on the edge of the tube, and a purified glass rod was used to push them into the fluid. In one tube in which this was done bacteria developed. In another case the tube was tilted so as to bring the fluid in contact with the neck, and here also development occurred. The third tube was undisturbed and remained pure. In another set of experiments wide-mouthed beakers were used, and were exposed for three-quarters of an hour. In all of these organisms developed. Test experiments showed that these organisms had entered during the exposure.

These experiments do not, however, seem to be satisfactory. We are told in the first case that the spray was placed a foot above and $3\frac{1}{2}$ feet distant from the tubes, and that fifteen ounces of carbolic lotion, 1–20, were used. During half the time a board was placed so as to throw the spray back over the flasks. Now if the flasks were only *beneath* the spray, a foot distant from it, during half the period of exposure—the result cannot be wondered at, for eddies would be produced by the spray which would drive unpurified dust into the vessels. If, however, the spray passed directly over the mouths of the flasks—if these were enveloped by it—it is difficult to conceive that the spray could have played for so long at such a close dis-

[1] Dr. Koch has found that the *spores* of Bacillus Anthracis can resist 1–20 watery solution of carbolic acid for a considerable time, though the fully developed organisms are at once destroyed by it. *Micrococci*, however, are very susceptible to the action of this antiseptic.

[2] Shown to the New York Surgical Society, November 25, 1879.

tance, and that so much lotion could be used without the entrance into the urine of a quantity of carbolic acid more than sufficient to render it sterile. But granting that the method is correct in these respects, Stimson sweeps the floor, raises *large masses of dust which he can see*, and expects that as these fall through the spray they will be soaked through and through, and any organisms in their interior be destroyed! It never has been asserted that a mass of filth falling through a spray can be soaked completely in a moment by the acid. We do not sweep the floors while performing a surgical operation.

All that I assert is, that under ordinary circumstances the spray is thoroughly efficient. From this point of view it is in the main a question of size of the particles which the spray meets with. If these are minute and but little compact, they will be disinfected. If they are large and dense, as will be the case if the floor be swept during or immediately before the experiment, one could not expect the spray to soak through them sufficiently during their transit. There is another way, however, in which the spray may act on these larger particles—viz. by bedewing the surface of the wound, and thus keeping up the action on the dust which began during its transit through the spray. In fact, the particle of dust already moistened while passing through the spray, falls into a thin layer of carbolic lotion, and thus disinfection is completed. As a rule, however, particles of dust which are small enough and light enough to float about in the atmosphere, such particles as are present in ordinary rooms or wards, will, as far as I can judge, be acted on directly in a sufficient manner by the spray, for they will not fall straight through it, but will be carried along with it after being moistened with carbolic acid, and thus time is afforded for the thorough action of the antiseptic before they reach the wound. If these organisms got in through fault of the spray, we can hardly imagine that they would always be micrococci. We must, therefore, examine the action of the dressing in order to see if any explanation can be obtained there. We shall see that it can, and that these organisms do enter on account of failure of the dressing to fulfil all requirements for a lengthened period.

As I have already shown, it is generally at a late period in the treatment of a case that these organisms appear; generally where the dressings have been left on for several days; or where the discharge has reached the edge some hours before changing it; or, if they occur early, it is when the discharge which has come through has been considerable, although the dressing has been changed early; and thus I have been able in some cases to prevent the entrance of these organisms by changing the dressings daily, and, conversely, they are easily enough obtained in any given case simply by dressing it less frequently. This will be readily seen from the cases quoted before.

If such a fluid as milk be tested with the view of determining the amount of carbolic acid which must be added in order to prevent the development of organisms, it will be found that a large quantity is necessary. Thus, they readily develop in a proportion of carbolic acid and milk, 1–60, and I have grown them in as large a proportion as 1–54. On examining the milk while the carbolic acid is being added, a granular precipitate will be seen to take place. In the same way, if carbolic acid is added to serum or white of egg, a white precipitate occurs, and here also a large proportion of carbolic acid is necessary to prevent development. In artificial cultivating liquids—such as Pasteur's fluid—no precipitate takes place, and a much smaller quantity of carbolic acid is required to hinder the growth of organisms. In other words, where carbolic acid is added to an albuminous fluid, a compound is formed which is but little antiseptic. So in the case of the antiseptic dressings, where there is profuse discharge—though the first which comes through may not be putrescible, yet very soon the carbolic acid is not present in sufficient quantity to prevent the development of organisms; and, as carbolic acid is very volatile, when a dressing is left on for several days, a considerable amount of carbolic acid will have also escaped by evaporation, and thus the meaning of the following rules of treatment, derived from experience, becomes evident: 'A dressing must not be too small.' 'The dressing must not be left on longer than twenty-four hours after the discharge has appeared at the edge.' 'In no case is it safe to leave a dressing unchanged for more than eight days.'

Experience has shown that any marked disregard of these rules will, in all probability, be followed by putrefaction in the wound.

But this still leaves unexplained why it is that *micrococci* only are found in wounds treated aseptically. At first the only hypothesis which I could think of was that micrococci can grow in fluids containing carbolic acid in larger quantity than those in which bacteria can develop. I have accordingly performed numerous and elaborate experiments to test this view, but I have been quite unable to find any such difference in the first instance. An observation which I made in the course of these experiments seems, however, to furnish the clue to the mystery. I had previously observed that where one flask was inoculated with bacteria and another with micrococci in like amount, that which contained the bacteria was, as a rule, muddy in from twelve to twenty hours (the quantity of fluid in each flask being ʒiij to ʒiv), while thirty to fifty hours elapsed before the fluid in the flask into which the micrococci had been introduced became opaque. But if carbolic acid, say in the proportion of 1 to 500, be previously added to this fluid, the result is just the reverse: the flask containing the micrococci becomes opaque in twenty-four hours, while that containing the bacteria does not become muddy till a later period. Following out this line of investigation, I have found that if micrococci and bacteria be introduced together into a cultivating fluid containing carbolic acid, the micrococci will develop rapidly, often to the complete exclusion of the bacteria. Where no carbolic acid is present, the result is generally the reverse; most forms of bacteria grow quickly, the micrococci being often apparently prevented from developing. But, it may be said, in the former case the bacteria became converted into micrococci. But if the same bacteria be introduced into a flask containing no micrococci, bacteria alone develop.

So in the room in which I work, I have never been able, without the aid of the spray, to transfer micrococci from one flask to another. For in the latter flask *bacteria* almost invariably developed. But if carbolic acid be previously present in the fluid, the operation may be done in the most leisurely manner, with practically a certainty of obtaining micrococci alone or chiefly in the second flask.

It is thus apparent that though bacteria and micrococci can grow in fluids containing like amounts of carbolic acid, yet the micrococci find these liquids more suitable for their growth than do bacteria; indeed, they may grow more rapidly in them than in fluids containing no carbolic acid at all, and therefore, when bacteria and micrococci fall together into discharge containing carbolic acid, the latter develop with much greater rapidity than the former, and may thus reach the wound long before them. If, however, sufficient time be allowed to elapse before the changing of the dressing, bacteria also may enter the wound.

One other observation completes this subject. The largest proportion of carbolic acid in cucumber infusion in which organisms develop is from 1–450 to 1–500. In one of the last experiments performed with the view of seeing whether micrococci could grow in a larger proportion of carbolic acid than that sufficient to prevent the development of bacteria, I used micrococci which were growing in a fluid in which a small quantity of carbolic acid was already present. This was done on April 14th. On examining the flasks on April 15th, I found one containing carbolic acid in the proportion of 1–400 quite opaque from the development in it of micrococci, while those in which a larger proportion of carbolic acid was present remained clear. On the same day I inoculated from flask 1–400 a new series containing carbolic acid in the following proportions: C 1–400, C 1–350, C 1–300, C 1–250. On April 16th, C 1–350 and C 1–400 were quite muddy from the presence of micrococci; while C 1–300 and C 1–250 were clear. That afternoon, a fresh series, C′ 1–350, C′ 1–300, C′ 1–250, C′ 1–200, was inoculated from C 1–350. On April 17th, C′ 1–300 and C′ 1-350 were muddy. Here the limit seems to have been reached. For though I have obtained slight development in carbolic acid and cucumber 1–275 and 1–250, this is not vigorous. As the micrococci grow in larger proportions of carbolic acid, they become much larger, and the grouping and mode of growth described by Mr. Lister are more evident.

The facts then seem to be that the discharge, when profuse, or when it arrives at the edge of a dressing which has been left on for some days, does not contain sufficient carbolic acid to

prevent the development of organisms in it; that micrococci, which I find to be *more abundant in the ward atmosphere* than bacteria, find this a particularly favourable medium for growth; and that as they grow they increase in vigour, and become more able to live in fluids containing a larger proportion of carbolic acid, and thus, if time be given them, they will eventually reach the wound.

Since these investigations were made, I have been able to *demonstrate* the spread inwards of organisms under a gauze dressing. Look for a moment at Case 12, p. 241. A specimen of the discharge was taken on March 31st, from the gauze dressing near the edge, and was found to contain *micrococci and bacteria.* No organisms had, however, as yet reached the wound (see Fig. 18, Plate III.). This dressing was removed, a new dressing applied, and the organisms had to begin again at the edge. Fig. 19, taken from the dressing near its edge, and Fig. 20, taken from the drainage tube four days later, illustrate the same thing. Here also the organisms were penetrating inwards under the dressing, but had not yet reached the wound, and it was not till two days later that the first traces of micrococci appeared in the wound.

The same thing is demonstrated in Case 13, p. 242. Here Fig. 21, Plate III. is a specimen taken from the gauze near the wound, and it contains *a few micrococci*: they had not, however, reached the wound, and did not do so till some days later.

Then, again, I have been able to keep out these organisms simply by dressing more frequently so as to anticipate them before they reach the wound, as may be seen in the case of empyema, &c. And Dr. Ogston does not find micrococci in his aseptic cases, because he dresses much more frequently than Mr. Lister, never allowing the discharge to appear at the edge of the dressing at all.

It will not, however, be always necessary that the discharge should appear at the edge, for if a dressing be left on for several days sweat accumulates under it, and will serve the same purpose as the serous discharge in conducting organisms inwards.

At the same time it is possible that organisms may occasionally enter the wound from the blood, especially in the conditions of interference with the healthy state which were

formerly alluded to; and it is also possible that they may enter from fault in the aseptic precautions, though I think this must be exceedingly rare. For it is difficult to imagine that in the latter case only micrococci would get in; indeed, in faulty experiments with cultivating fluids, it is almost invariably some form of rod-shaped organism which appears.

CHAPTER XIII.

ANTISEPTIC SURGERY.

Complete definition of antiseptic surgery. Varieties of antiseptic surgery. TREATMENT BY ANTISEPTICS: Carbolic acid—objections to it: Chloride of zinc: Boracic acid: Sulphurous acid: Chlorinated Soda: Alcohol—Hutchinson's method: Terebene and Sanitas—Bilguer's method—Neudörfer's salicylic powder. FREE DRAINAGE AS AN ANTISEPTIC METHOD. IRRIGATION AND IMMERSION. OPEN METHOD: Modes in which it acts antiseptically: Bartscher and Vezin's method: Burow's method: Rose's modification. HEALING BY SCABBING: Methods of forming a crust: Bouisson's ventilation method: other modes. GUÉRIN'S COTTON-WOOL DRESSING. MODES IN WHICH THE DESTRUCTIVE ACTION OF THE TISSUES ON BACTERIA IS ASSISTED. Why does not fermentation always occur in the blood in wounds in which organisms are present? Best practical methods. Conclusions.

WE have now arrived at the end of our discussion as to what are the particles which cause putrefaction; what are the exact enemies with which we have to contend in attempting to prevent putrefaction. We have seen that it is from particles falling into fluids or on tissues that organisms develop. We have seen that it is only after the access of particles from the outer world to such fluids and tissues that fermentations occur, and we have satisfactorily demonstrated that the particles which cause fermentations and those which give rise to organisms, are one and the same—in other words, fermentations are due to the growth of organisms in fluids or tissues. We have also seen that these same laws, with one exception, to be presently mentioned, hold good when the fluids or tissues are confined in the living body, just as when they are in flasks, viz., that the particles which cause putrefaction and other fermentations only rarely enter such substances through the circulation, but generally reach them directly from the air or from surrounding objects; that so long as an animal is healthy, dead

fluids or tissues may remain unfermented in closed cavities in the body, and may even entirely disappear, but that as soon as atmospheric air with its dust is admitted, organisms develop, and fermentations occur. We are now therefore able to take a much wider view of the meaning of the term 'Antiseptic Surgery' than is generally done. It is no longer surgery which only *excludes* the causes of putrefaction; we may now include under the term *all those methods of wound treatment in which, wittingly or otherwise, the growth and fermentative action of the lower forms of organisms (bacteria) are more or less impeded.*

When we come to look at the numerous methods of wound treatment from this point of view, we shall see that there is perhaps none at present employed, with the exception of the poultice dressing so much lauded by Mr. Savory, which does not in some way or other, however imperfectly, interfere with the growth and fermentative action of bacteria, and which does not owe its chief virtues to that cause.

I shall not enter into details on all these methods, for their number is legion, but I shall discuss the modes in which this interference with bacteric action may be carried out under several headings, and indicate briefly the principles which ought to guide the surgeon in carrying out one or other method.

The antiseptic methods which merely *interfere* with the development and fermentative action of organisms on the fluids and tissues in wounds, and which do not aim at their total exclusion, may act on various principles.

1. By the addition of various antiseptics to the discharge, either in the wound, or after it flows out, this discharge may be rendered an unfit soil for the development of organisms.—*Use of antiseptics.*

2. The discharge may be allowed to flow away so rapidly as not to have time to undergo fermentation to any extent in the wound itself.—*Free drainage.*

3. This removal of the discharge may be facilitated by washing it away constantly with water alone, or with water containing antiseptics.—*Treatment by irrigation or by water bath.*

4. By freely exposing the discharge to air evaporation takes place, and the fluid becomes too concentrated to permit the growth of bacteria, while, at the same time, by supplying these organisms with plenty of oxygen, they have no necessity to break up the albuminous compounds in their search for oxygen, and thus, as shown by Pasteur, their fermenting power is diminished.—*Open treatment.*

5. By keeping the parts at perfect rest and by operating only when the patient is in good health, the tissues and the blood are in such a state as to resist the development of bacteria in the thin layer of lymph between the cut surfaces, and union by first intention thus occurs. This is best carried out *by perfect rest and accurate apposition of the cut surfaces.*

Healing by scabbing acts on the last two principles.

Although these various methods may be described as acting on these different principles, yet there is no hard and fast line between one and the other. Indeed, at the present day advantage is now constantly gained from the use of the various principles combined—as, for instance, by the employment of free drainage, antiseptic irrigation, &c., in the open method. As these methods are so very numerous, and as they are generally modified by every surgeon who employs them, few indeed using them on the true principle, I think it will be best merely to make a few remarks under each heading, and then when we come to the historical part we shall be able to fill up blanks in the following description.

I.—*Methods by which various Antiseptics are added to the Discharge, so as to hinder the Development of Organisms in it.*

What are the best antiseptics to use for this purpose?

Carbolic acid is the one most frequently employed, but, in my opinion, it is by no means the best in this instance. We have already seen that in vegetable infusions, where carbolic acid is present in the proportions of 1–200 to 1–250 all further growth of organisms is prevented, but that in such fluids as serum, milk, pus, &c., the acid forms a compound with the albumen, and a much larger proportion is required. Thus, in

milk, carbolic acid in the proportion of 1–54 is just enough to prevent development. In accordance with this fact, a very strong solution would be required in the case of wounds, or it must be added in large quantities, in order to prevent the development of micro-organisms.

And so my own experience of carbolic acid as a disinfectant in the form of a 1–40 watery solution in putrid cases is unfavourable, while, on the other hand, if 1–20 carbolic acid be used, it is very irritating, and interferes with healing. Injected once or twice a day, the latter destroys the superficial granulation cells, and produces a thin slough in which bacteria develop, and from which it is very difficult to dislodge them. Then its poisonous qualities are objectionable, and are of course much more evident when the acid is injected into wounds or abscess cavities than when used in the manner described in the chapters on aseptic surgery.

Further, Dr. Wilhelm Hack,[1] in a paper on the power of absorption by granulations, has demonstrated that granulations treated with carbolic acid possess many of the qualities of a recent wound as regards absorption. For instance, apomorphia, which was only absorbed by wounds treated with water dressing during the first twelve hours, was readily absorbed at any time by granulating wounds treated with carbolic acid; and therefore in the absence of information to the contrary, I should fear that some of the poisonous products of putrefaction might be absorbed with like avidity. Hence, I do not like carbolic acid unless it is used aseptically.

Chloride of zinc applied to the cut surface has been already alluded to. A single application has the remarkable property of preventing putrefaction in a wound for some time after an operation; sometimes, indeed, till granulation is nearly complete. It is further useful, according to Hack's experiments, in that the slough caused by it does not permit the absorption of substances from the wound. When used in the treatment of wounds, a dilute solution (1 or 2 grs. to the ounce of water) is employed.

Boracic acid is too weak an antiseptic to be of much service

[1] Ueber das Resorptionsvermögen granulirender Flächen. Leipzig, 1879.

as an injection, but the boracic ointment and the boracic lint act well as dressings.

One of the best antiseptic lotions is made with *sulphurous acid.* This is a powerful germicide. It is also non-irritating and perfectly free from any poisonous qualities. It is used as a solution which is made by mixing together equal parts of the sulphurous acid of the pharmacopœia and water or glycerine. This may be still further diluted if necessary.

The *chlorinated soda* solution is mentioned by Dr. Cabot as standing next to 1–20 carbolic lotion in rapidity of action on bacteria. The strength generally employed is ʒss. to ℥j of water.

Alcohol is not a bad antiseptic, but in order to be effectual, it must be used strong. It has a further advantage, for Dr. Hack has shown that granulations treated with alcohol do not absorb at all or only very slightly; and to this may be attributed, to some extent, the favourable course of the cases in which wounds are simply washed out with an alcoholic solution, and a rag, dipped in the same solution, applied outside.

Mr. Jonathan Hutchinson, more especially, has had remarkably good results from the use of alcohol. His method is as follows:—Having carefully arrested all hæmorrhage, chiefly by torsion, he washes out the wound with pure spirit. He then carefully arranges drainage tubes at the most dependent parts, and stitches up the rest of the wound. Thin compresses soaked in a lotion composed of 6 parts of absolute alcohol, a half part of liquor plumbi, and 16 parts of distilled water, are now applied. These compresses are kept constantly moist, either by a nurse or by means of a drop irrigator. The lint is changed daily. In the treatment of important cases, such as compound fractures and dislocations the rule observed is, never to allow the skin to become warmer than natural. His wounds generally heal by first intention, and septic poisoning is very infrequent.

In some of these cases no doubt the wound may be aseptic from first to last. To apply an antiseptic to a wound, to stitch up immediately, and then keep an antiseptic dressing constantly applied, is really to operate more or less aseptically, and I believe, with regard to Mr. Hutchinson's cases, that this

partly explains the good results—the wound being aseptic, at least for a time. Then Hack's results have a strong bearing on these cases, for absorption does not take place readily. Mr. Hutchinson is also very particular to have free drainage, which powerfully helps to maintain the aseptic condition; and lastly, he carefully selects the cases for operation, only operating, unless in cases of necessity, where the patient is in good health. This selection of cases is a thing not necessary and not done where complete aseptic treatment is employed.

The method which I should think was the best, acting on this principle, is the following; it is practically what Mr. Lister employs with excellent results, only I would reject the carbolic acid. After the wound has been made, and before any stitches are inserted, the raw surface ought to be thoroughly sponged over with chloride of zinc solution (40 grs. to the oz. of water). In the case of operations on the extremities, this is best done before the tourniquet is relaxed, so as to ensure its thorough application, for otherwise the blood would wash away the solution or dilute it before it has had time to act. Silver wire stitches are then inserted—special care being taken to ensure free drainage, by the use of large drainage tubes. As a dressing in the first instance, till the bleeding has stopped, several layers of wet boracic lint (wet in boracic lotion) are applied.

On the day following the operation, the lint is removed, the surface of the wound is thoroughly cleaned with sulphurous acid or chlorinated soda lotions, or with Hutchinson's lotion, and the drainage tubes are washed out with the same, though not removed. The dressing is now a narrow strip of the salicylic, eucalyptus or full strength boracic ointments, thinly but evenly spread on calico, and outside this, overlapping it in all directions, one or more broad layers of boracic lint.

On the second or third day, the drainage tube is removed, and is washed in 1-20 carbolic lotion, the wound being then syringed out with the sulphurous acid or other lotion. After a day or two the ointment over the line of incision is changed to the half-strength boracic, or if salicylic or eucalyptus ointment was used, they are retained. These dressings are changed daily

at first, but when the discharge diminishes, they may be left for two days.

Terebene and *sanitas* are remarkably good applications where the smell is bad.

The results of this treatment are of course not so perfect as those of the aseptic method, for, however carefully one washes out the wound, there are pouches in it into which the fluid does not enter, and pieces of slough cannot of course be disinfected. Thus, prolonged suppurations may occur, caries may continue without tendency to cure, and even accidental wound diseases (pyæmia, &c.) attack the patient.

With regard to the use of chloride of zinc, I ought to say that it is well not to apply it to wounds which must, if possible, heal by first intention, as, for instance, in incisions about the lips or face.

It was on this principle, as we shall see, that Lemaire employed carbolic acid and coal tar; and his results, though very good, by no means correspond to those obtained by strict aseptic treatment.

It was also on this principle that good results followed the use of balsams of various kinds in olden times. The most remarkable example of the success of such attempts at rendering the wound secretions incapable of putrefaction by the use of balsams, was that of Bilguer in the last century. No doubt where the wound is shallow, and possesses few recesses, and where the balsam or other antiseptic employed fills up these recesses, we have really an aseptic treatment and an aseptic result.

By sprinkling powdered salicylic acid on wounds till no more fluid passes out, Neudörfer manufactures a paste under which he says that healing may occur without suppuration.

II. *On Free Drainage as an Antiseptic Method.*

I have already discussed the main principles of drainage under the head of aseptic surgery. It is quite clear that, if discharge flows away as fast as it is formed, there can be no marked development of bacteria or of their products. The free drainage

of a wound from which organisms are not from the first excluded is therefore of the utmost importance. I have already described the use of india-rubber tubes, and I have referred to catgut and horse-hair. Since, in a wound not treated aseptically, fermentation, most probably followed by suppuration, generally occurs in the track of the drain, we must provide such a drain as shall permit the free escape of pus. Now, neither horse-hair nor catgut can drain pus, and, therefore, a tube of some kind or other must be used. This may be an india-rubber one, or it may be made of various kinds of metal, perforated at its sides, and cut flush with the surface. The tube, of whatever material, must be removed from the wound at each dressing and washed with a strong antiseptic lotion, say 1–20 carbolic lotion. If this be not done, portions of decomposing tissue, &c., remain inside the wound, and become more and more putrid till very soon they become caustic.

Where the wound is not treated aseptically, the principle of having the most dependent opening possible must be carried out to the full.

III. *Irrigation and Immersion.*

The principle of free drainage is never of course used alone, other principles act along with it. Of these, one of the most satisfactory is that in which the discharge is not merely allowed to flow away, but is washed away, and the further addition to this principle of adding an antiseptic to the water used for the irrigation and of thus keeping the wound constantly bathed in an antiseptic fluid. The latter is the form in which irrigation and the water bath are now always employed, viz., by the use of an antiseptic solution.

Irrigation is, as a rule, only practicable on the extremities, though it may be carried out on the trunk. For the latter, however, the continuous water bath is the most convenient.

The wounded part having been arranged at perfect rest, a sheet of mackintosh is fastened to the limb, and so arranged that the fluid flowing from the wound shall be conducted to a tub; the vessel containing the fluid is fixed at a considerably higher level than the patient. The form of irrigator most

generally used at the present time is Esmarch's. This consists of a cylindrical leaden or zinc vessel, which has a ring at its upper part to enable it to be affixed to the wall. From the side of this vessel, close to its bottom, a tube passes, and to the end of this tube is fastened a long piece of india-rubber tubing with a nozzle at its end. This nozzle is arranged so as to direct the fluid into the deeper parts of the wound. The fluid used is generally some weak antiseptic solution, sueh as chlorinated soda or sulphurous acid, or boracic acid.

A very good apparatus ean be made in an emergeney (aceording to Thiersch) by knocking the bottom out of a champagne bottle, and having the tube for conveying away the fluid passed through the cork. The bottle is inverted, filled with the solution, and fastened to the wall. The fluid used may be tepid or cold, as we shall see later. There is no advantage in using it very cold, as recommended by some.

Where the fluid is dropped on to the wound, it is well to place a piece of lint over the part where the drop falls, to prevent the constant irritation caused by the concussion. The skin in the neighbourhood of the wound ought to be coated with palm oil, in order to prevent maceration.

The continuous bath is either a bath in which the whole patient can be immersed, or one in which the wounded part alone is placed. There are numerous methods of doing this, and the references to these will be given in the history of the subject.

FIG. 73.—THIERSCH'S CHAMPAGNE BOTTLE IRRIGATOR.

The advantages of the treatment by constant irrigation are, that the discharges are removed as fast as they form, and at the same time, where an antiseptic is employed, the part is kept constantly sweet. Thus, where the cavity is small and uncomplicated, there may be a truly aseptic state of affairs.

At the same time, where tepid water is used granulation is favoured, while pain and nervous irritation are very much

diminished. Further, the parts are kept at absolute rest, the necessity of moving them in order to change dressings, &c., being avoided.

As an antiseptic means, I should think that satisfactory irrigation is better than the continuous water bath, for in the latter there is not the same constant change of fluid, nor the same washing away of the discharge.

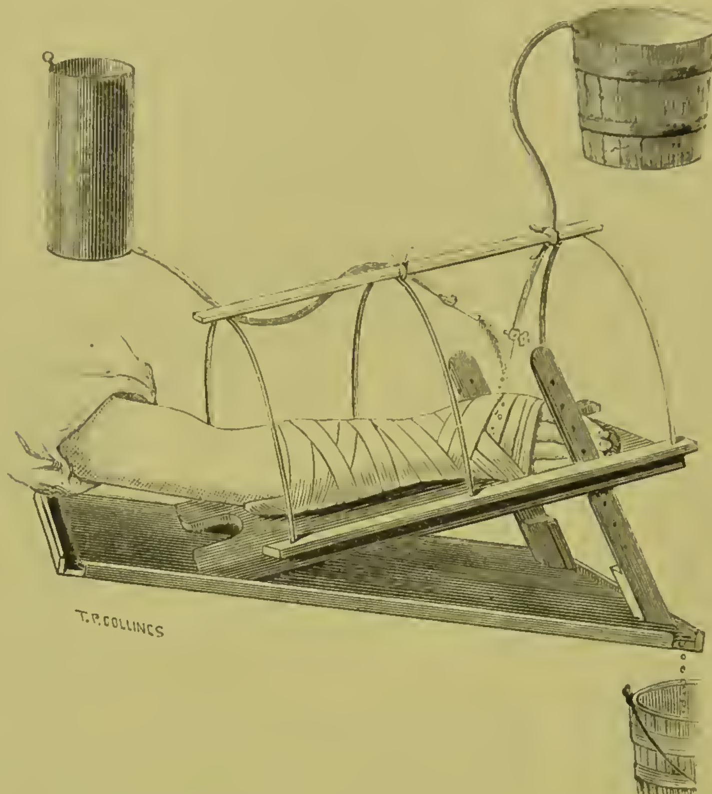


FIG. 74.—ARRANGEMENT FOR IRRIGATION IN THE UPPER LIMB. (After Esmarch.)

The favourable results of constant irrigation in preventing septic diseases are very remarkable, and, in this respect, it probably stands next to strict aseptic treatment.

The disadvantages of these methods are for the most part the sodden state of the wound and the consequent œdematous condition of the granulations, the constant state of unrest of the wound and the inconveniences attending the application of the method. For the first reason they are discontinued when granulation is complete and has filled up the deeper parts of the wound, and when the time for blood poisoning has passed.

That the good results of irrigation and immersion are not due to mere maintenance of temperature, as has been

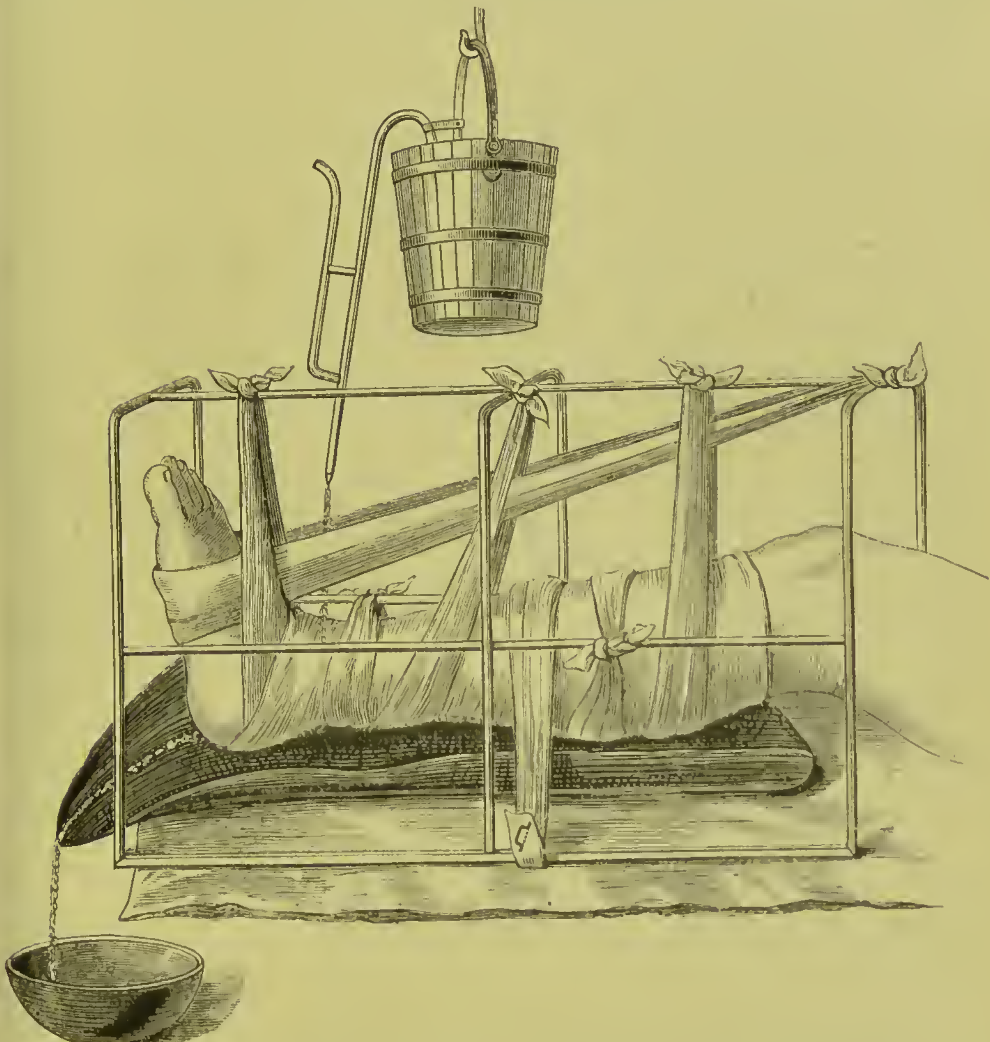


FIG. 75.—ARRANGEMENT FOR IRRIGATION IN THE LOWER LIMB. (After Esmarch.)

supposed, is shown by the results of M. Guyot with his incubation method. Here the wounded part was enclosed in

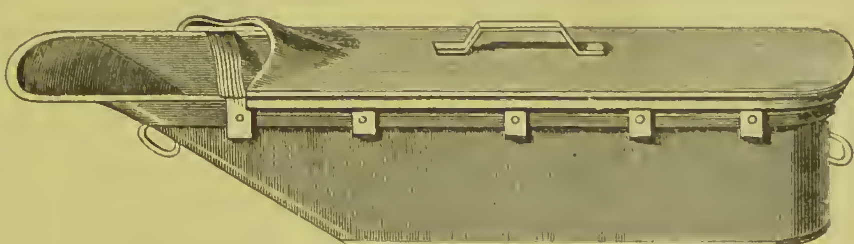


FIG. 76.—APPARATUS FOR CONTINUOUS IMMERSION. (After Esmarch.) (For the extremities.)

an incubating apparatus, and kept permanently at a high temperature. This method, though much lauded for a time, really seems to have favoured considerably the development of septic diseases.

IV. *Concentration of the Fluids, and their Admixture with Oxygen.*

The method of treatment which has been the greatest stumbling-block in the way of the acceptance of the principles of antiseptic surgery is the open method, for surgeons have been unable to see how the success of this method could be reconciled with the germ theory of putrefaction. They have looked on it as the antithesis of aseptic treatment, as acting on the very opposite principle to that on which the aseptic method is based. And yet, when we come to consider the matter in the light of the true principles of antiseptic surgery, we find that the open method is an advanced method of antiseptic treatment. Of course other principles, such as that of perfect rest and free drainage, also tell markedly in this case.

I have stated that this open method acts antiseptically in two ways, and these I must now briefly consider.

1. It acts antiseptically in that the discharges dry up, and become more concentrated, and thus become unfit soil for the growth of bacteria.

That concentrated fluids are not suited for rapid development of organisms is well known. Thus Pasteur pointed out that organisms could not grow in sugary solutions which had become concentrated. This fact is made use of in the arts, in the preservation of fruits. Sugar is added in large quantities, and then the fruits can be kept for an indefinite length of time. It is not that the sugar is an antiseptic killing the organisms, it is merely that by its presence in large amount the fluid is rendered unsuitable for development.

The same principle is made use of in preserving milk. The milk is evaporated to one-third of its original volume, and a considerable quantity of sugar is added to it. Without the addition of the sugar, the condensed milk may be kept for a

considerable length of time without the appearance of organisms in it; with sugar it may be kept indefinitely.

The same is the case with other albuminous fluids such as the discharge from wounds. Concentrate pus, and it will be found that organisms develop in it only with difficulty.

Then we know the contrast between cases of dry and moist gangrene; how in the latter putrefaction rapidly occurs, or, in other words, organisms rapidly develop, while in dry gangrene putrefaction does not occur, i.e. organisms cannot develop.

The same was seen in Cazeneuve and Livon's experiments on urinary bladders, mentioned before at p. 37. In that case organisms could not develop in the wall of the bladder, because the fluid was constantly evaporating.

2. In the open method another antiseptic advantage is gained by the free admission of oxygen to the discharge.

Some very remarkable effects of oxygen in retarding putrefactive and other fermentations were published long ago by Pasteur. He pointed out that if a sugary solution were freely exposed to air in a thin layer, the yeast plant, though it grew luxuriantly, caused very little fermentation. On the other hand, if oxygen were excluded, only a small development of the yeast cells was necessary for fermentation. And he has shewn that other plants besides the yeast plant can cause alcoholic fermentation, if only they are deprived of free oxygen.

With regard to putrefaction he has brought forward similar evidence. The organisms which cause putrefaction are, according to him, incapable of living in the presence of oxygen. If a putrescible fluid be freely exposed to the air in a thin layer, putrefaction does not occur, at least not for a very considerable time. Just as in the butyric fermentation, oxygen not only interferes with the fermentative process, but actually destroys the bacteria which cause it. Hence the free exposure of a putrescible fluid to the air results in comparative freedom from putrefaction, partly because the oxygen interferes with the development of fermentative changes, and partly because the oxygen directly kills the putrefactive bacteria.

There are two methods of wound treatment which act on the principles alluded to above. The first is that introduced by Bartscher and Vezin, and carried out in the following manner: 'After

all bleeding vessels have been tied, and after the stump has been cleansed from blood clots by means of a sponge and cold water, the patient, for whom two beds are provided close to each other, is put to bed, the stump is laid on a soft pillow, and over it a piece of gauze or linen is loosely placed in order to keep out the flies, the whole stump being freely exposed to the air.' At the morning visit, the surgeon pushes his hand under the stump, raises it, removes the pillow and applies a new one, or lifts the patient on to the other bed, without further cleansing of the wound. In this method all attempts at union by first intention are given up.

Burow of Könisberg published his method in 1859. He attempted to obtain primary union as far as possible. This he did by bringing the surfaces of the wound into contact after a few hours by means of strips of plaster applied over part of the surface, all other dressings being avoided.

Rose, whose results of open treatment have been specially published by Krönlein, adheres to Bartscher and Vezin's method, with this exception, that he daily washes out the wound with some antiseptic lotion. Crusts are removed, as they are never complete enough to prevent putrefaction, and only cause tension by their presence, and indeed protect the discharge underneath from the action of the air, or of the antiseptic lotion. Rose farther ventilates very freely, so as to have a plentiful supply of air to the stump. He employs this method in all wounds, except those on the face, where union by the first intention is desirable.

The open method pure and simple is that introduced by Bartscher and Vezin; for here, while the discharges are allowed to flow away freely, they are left to the unaided action of the air. The antiseptic effect of this method is no doubt increased by the addition of intermittent antiseptic irrigation.

Burow's method cannot act nearly so perfectly, for in it discharges must accumulate in various parts of the wound, and they are therefore not so conveniently placed for thorough action of the air.

Among the objections to these methods are the following; in all, except Burow's, union by first intention cannot possibly occur, indeed no attempt is made to obtain it; as a conse-

quence of this, a long time is required for healing, while a larger scar is obtained; the frequent formation of scabs and the consequent tension also cause great annoyance.

Closely allied to the open method stands *healing by scabbing.* This may be brought about chiefly in two ways. The crust may either be allowed to form naturally, or its formation may be aided by artificial means.

This healing by scabbing acts in two ways. In the first place, the first principle of the open method of treatment comes into play; the discharge dries up and becomes an unfit soil for the development of organisms. In the second place the fluid underneath it is in such a thin layer that the living tissues in the neighbourhood prevent the development of organisms in it. Of course in many cases no living organisms would be there to develop, for the scab would form an absolute protection against their entrance.

The natural formation of the crust can only take place efficiently in small wounds, and only exceptionally where cavities and recesses are present. For in larger wounds, as we have seen, there is too much discharge for a sufficiently rapid and thorough formation of a crust; and if the crust is not perfect and rapidly formed, putrefaction takes place underneath it, while at the same time, by confining the discharges, tension and ulceration result in place of healing.

But though this natural formation of a crust cannot be trusted to in most cases, yet by artificial means a satisfactory one may be obtained.

The method in which this was done by Bouisson, was by blowing air on to the wound, and thus drying the discharges. This method will be described in the historical part of this work.

The most common modes are by the application of various powders, such as starch, alum, flour, &c., to the discharge, so as to form a paste. The best substance which can be employed in this manner is powdered salicylic acid, as recommended by Neudörfer, and mentioned before under 'Treatment by Antiseptics.' This is sprinkled on till no more fluid exudes, and it combines the advantages of a thorough crust with those of an antiseptic dressing.[1]

[1] For details of these various methods, see the historical part.

Other attempts have been made to produce a scab by the application of various caustic substances to the surface of the wound. The crust so formed, containing as it does a strong caustic, is unable to undergo putrefaction ; and where the inflammation caused by the application is not too great, and does not lead to accumulation of fluid under the crust, an excellent result is obtained. In this case, as no organisms are present under the crust (they were destroyed by the caustic), and as the firmly adhering crust prevents their entrance, we have the typical aseptic result—healing of an open wound without suppuration or granulation. There are, however, various objections to this method of crust formation, the chief of which is the loss of substance involved in the process, while the same disadvantages are present as in the open method proper, viz., slowness of healing and a large scar.

Closely allied to this method of crust formation is that in which the crust is obtained by the application of the actual cautery.

Crust formation is seldom suitable unless in the case of superficial wounds without recesses or cavities, for, if these are present, an opportunity is given for the occurrence of tension under the crust. It is a method by no means easy or universal in its application. Where it is employed I should recommend the use of Neudörfer's method, viz., the formation of a crust by the aid of salicylic acid powder.

Alphonse Guérin's Cotton-wool Treatment acts partly on the first part of the principle of the open method, but it hardly comes into the category of antiseptic methods.

As originally used, the wound was simply washed with water, and a large mass of cotton-wool applied around it, and firmly bandaged on.

At present the wound is washed with some antiseptic lotion, and layers of cotton-wool, containing camphor powder sprinkled in it, are applied. The deeper layer consists of wool moistened in carbolic lotion.

More details of this method will be found in the historical part ; but I may mention here a point which Guérin considers of great importance. The dressing should not be applied or changed in the ward, but in a theatre or side room, and the

packet of cotton-wool should not be opened till the time of application of the dressing. No doubt these precautions may prevent some infective material present in the ward atmosphere from settling on the wound or on the cotton-wool, but it is only a chance that such will be the case.

This method can hardly be called 'antiseptic' in the sense in which we have used the word, for the antiputrescent principles on which it acts are not very powerful. As the result of these dressings, the discharge becomes thick and concentrated, and not a very good medium for development of organisms. Nevertheless this concentration of the discharge cannot occur to anything like the extent which takes place when it is left freely exposed to the air. The second principle on which it acts is that it ensures absolute rest to the wound. By means of this rest the granulations are not lacerated, and neither bacteria nor their products can be admitted into the body. This however is a principle relating to *infective* disease, a subject which we have not mixed up with the true antiseptic principle, which is solely that of preventing putrefaction in the wound.

Guérin's method cannot be recommended except in some exceptional cases of disease of joints with sinuses, and even here the retention of the putrefying discharges, and the consequent irritation, render it of very doubtful value.

I have already described the aseptic applications of pure cotton wool at p. 141.

V. *Assist the destroying Action of the healthy living Tissues on Bacteria.*

This principle may be aided in two ways, viz., by perfect mechanical rest, and by attention to the general health.

By perfect mechanical rest, when the tissues are in perfect health, and the blood clot is undisturbed, the tissues and clot may be kept in such a state as to resist the development of organisms. This method, though without recognition of the antiseptic principle, has been long practised, and of late has been specially advocated by Sampson Gamgee.[1]

[1] *On the Treatment of Wounds*, 1878.

It is, of course, a well-known fact that, without any antiseptic appliances at all, wounds, more especially about the face, heal frequently by first intention. How can this take place if blood or lymph, exposed to the air, putrefies as the result of the access of organisms? For, during the operation, organisms enter the wound both as dust from the air and surrounding objects, and also from the water in which the sponges are soaked. There is also between the cut surfaces a layer of blood or lymph (which, however, must as a rule be very small in amount, otherwise healing by first intention does not occur) which, if it were exposed between two plates of glass, would probably putrefy in a short time. How is it that union by first intention can occur under these circumstances? How is it that the blood does not putrefy between the cut surfaces of a wound?

Well, to use Mr. Lister's forcible arguments, the *fact* is that a thin layer of blood, although containing numerous causes of putrefaction, does not as a rule putrefy if it be placed between two healthy living cut surfaces. Or, to state the fact in another way, these organisms, which are certainly present, cannot develop in a thin layer of blood or lymph placed between two healthy living freshly cut surfaces. Or, to state the same fact differently (taking into consideration the different results when the same layer of blood or lymph is placed between two plates of glass), the *living tissues, when in a healthy state*, have the power of preventing the development of organisms in their immediate vicinity.

I have already had occasion, in a former part of this work (p. 252), to point out that if into a *healthy* living animal a small quantity of ordinary bacterial fluid be injected, the bacteria lose their vitality and disappear. I have shown how organisms cannot be found in the living healthy body (I except here of course specific pathogenic organisms, such as bacillus anthracis), unless a considerable amount of their products be introduced along with them. On the other hand I demonstrated how, if the animal were out of health, organisms could live in their blood and tissues much more easily. The same is the case in union by first intention. If the part be of high vital power, and in a healthy state, and if there be an

extremely small amount of blood or lymph between the cut surfaces, union by first intention will almost eertainly oeeur. If the part become inflamed, or if the patient be in a weak state of health, union by first intention, without aseptic means, becomes a matter of great uncertainty.

Traube and Gseheidlen[1] have likewise found that blood taken with precautions from a healthy living rabbit into which, 24 or 48 hours previously, $1\frac{1}{2}$ e.em. of baeterial fluid had been injected, eould be kept for months without undergoing putrefaetion.

And, further, the facts whieh I have already mentioned, that the blood and tissues of healthy living animals do not eontain living organisms, shew sufficiently that they have the power of destroying them, for otherwise there are frequent opportunities for the entrance of these into the circulation.

This same prineiple was made use of by Mr. Lister in his experiments for obtaining unboiled urine (see p. 36). He simply washed the glans penis, and the meatus urinarius. He did not wash out the urethra at all. The urine passed in this way remained absolutely pure, showing that no organisms were present in the urethra. And yet the urethra eontains mueus, putreseible outside the body, and there is suffieient time between the acts of micturition for bacteria to spread quite up to the bladder (this was experimentally determined by Mr. Lister); nevertheless they do not penetrate any distance into the urethra. In other words, they eannot develop in this putrescible mueus, when it lies between two healthy living surfaees. Or, to state the fact otherwise, the healthy living tissues have somehow or other the power of preventing the development of organisms in their immediate vicinity.

Now blood clot itself may be looked on as a tissue, though one which is of very low vitality. That it is a tissue is well shown by Mr. Lister's experiments on eoagulation of blood; for he found that a tube or cup of blood clot aeted in the same way as regards the prevention of eoagulation as the living walls of the vessels. He further found that movement of the clot leading to its laeeration destroyed its vital power as a tissue.

In the 'Dublin Medical Journal' for August 1879, Mr. Lister, after referring to this subject, mentions the following facts:—

[1] *Loc. cit.*

'I have found, in experiments not yet published, that you may put into such purified glasses' (containing pure blood) 'drops of water of considerable size; and although every hundredth of a minim probably contains a septic organism, and although the blood that flows into each glass is mixed with this septic water, yet the clot there formed, if protected from any other source of disturbance afterwards, remains sweet and pure for weeks together, proving that even outside the body the blood clot has the power in itself of resisting the development of septic organisms—a most important truth.' After applying this principle to union by first intention in cases of amputation, he says: 'But suppose such a stump attacked by muscular spasm churning up the blood clot, then the septic particles would develop, and then you would have putrefaction and septic suppurations, so that without antiseptic treatment in this kind of case, you are at the mercy of perfect immobility; and, as I have said before, even with the best means, you cannot secure this.'

In his recent address at Cambridge, Mr. Lister refers to similar facts, but as he has not yet published them, and as the accounts given in the journals are very garbled, I cannot refer to them. It seems however that he confirmed the experiments just mentioned, and found that blood clot, when kept perfectly undisturbed, did not putrefy, even if 4 drops of tap water were placed in the flask before the blood was introduced.

A very interesting experiment bearing on this subject is mentioned by John Hunter in his paper on 'The living principle of the blood.'

After pointing out how fresh and living eggs resist putrefaction, and referring to various facts which he thought were indications of the existence of a living principle in blood, he narrates the following remarkable experiment, showing that the *blood of young persons resists putrefaction longer than that of old people*; in other words, that the blood of young persons has greater vital power.

'June 24th.—Some blood was taken from a woman twenty years of age, and its surface, after coagulation, was covered with an inflammatory crust.

'On the same day some blood was taken from a woman at

sixty, when the crassamentum was also covered with an inflammatory crust.

'These quantities of blood were set by.

'The blood from the old woman putrefied in two days. That from the young woman kept quite sweet till the fifth day, when it began to smell disagreeably; in this state it continued two days more, and then emitted the common odour of putrid blood.

'Several experiments were made in the course of the summer of a similar nature with the last, in all of which it appeared that the blood from young people kept longer sweet than that which was taken from the old.'

That healthy blood clot can resist putrefaction so long as it is kept at rest explains many remarkable cases which would otherwise seem at first sight at variance with antiseptic principles. The following case, showing the contrast between blood clot when kept at rest and when disturbed, is worth narrating.[1]

'On September 15, 1870, a young officer whose left upper arm had been broken about its middle by a Chassepot bullet at Noisseville seventeen days previously, arrived with an ambulance train at the Tempelhof military hospital.

'The plaster of Paris apparatus, which had been applied at once on the battle-field, had become soft and broken, and as the patient complained of pain in the arm, probably as the result of the journey, and as it was somewhat swollen, the apparatus was removed, and a careful examination was made of the wound. When I passed my finger into the wound after removal of the crust, I felt numerous fragments of bone, but was, however, astonished to find *no trace of pus, only coagulated blood.*

'After I had removed all the fragments, a plaster apparatus with a window in it was applied; nevertheless, there now resulted a *violent inflammation and suppuration of the wound*, which for a time threatened the young man's life.'

These views sufficiently reconcile the fact of union by first intention in cases not treated aseptically with the germ theory of putrefaction.

Now in order to have tissues in the state in which they are capable of resisting the development of bacteria in their immediate vicinity, they must be as much as possible in a condition

[1] Esmarch, Langenbeck's *Archiv*, vol. xx., p. 169.

of perfect health. To attain this, the health of the patient must be attended to and kept good, and all causes which irritate and cause the wounded part to inflame or become weaker must be avoided. The causes which weaken the part are various forms of unrest, mechanical or chemical.

To carry out this principle two things are required, viz., accurate approximation of the cut surfaees, and absolute immobilisation of the part. Where accurate approximation of the eut surfaces cannot be obtained, the same principle of absolute rest must be carried out as regards the clot filling up the gap, and it may thus resist the development of bacteria in its substance. Were I compelled to treat any case on this principle alone, I should combine with it the open method, leaving the wound freely exposed to the air. The superficial layer of the clot, by drying up, would thus to some extent form an obstacle to the entrance of organisms.

I have mentioned this as an antiseptic method, as the principle is of great importance in explaining certain otherwise puzzling cases, but I should not recommend it for adoption, for it is only in a few cases, such as in face wounds, where the vitality of the part is high, that this vital action of the tissues and blood elot can be sufficiently trusted.

Such are the chief principles on which antiseptic surgery can be carried out. In the class of antiseptic methods to which I have referred in this chapter, the surgeon does not adhere strietly to one or other principle, partly because the principles on which he acts have not as yet been properly understood or appreciated, and partly because better results can be obtained by their combination.

In many minor ways the antiseptic principle may be aided. Thus, by the use of catgut ligatures, we do not have a long septic thread hanging out of the wound, conducting putrefaction into its interior and leading to deep-seated suppurations, &c.

The silver suture acts in the same way as compared with the silk. Silver does not absorb the putrescible materials, and thus putrefaction does not occur in it. On the contrary, silk absorbs blood and serum, which putrefy in it, and the silk which was

at first unirritating, becomes very acrid and causes inflammation in its vicinity.

What are the various means by which the general health and tone are kept up in septic cases, what is the careful selection of healthy individuals for operation, but imperfect attempts at antiseptic surgery?

What is ventilation but an antiseptic means? The air being constantly changed, the foul emanations from wounds containing septic bacteria are diluted and swept away, while at the same time a better state of health is obtained.

And so I might go on enumerating various minute points which have been, in ignorance of their true significance, adopted in the treatment of wounds, all of which, more or less, act on and promote antiseptic principles.

We thus come to the end of our discussion of the Principles and Practice of Antiseptic Surgery. We have seen that antiseptic surgery is simply a struggle with the causes of putrefaction. I have not mentioned the germ theory of infective disease at all. That has no essential bearing on the *principles* of antiseptic surgery. All that is required of antiseptic surgery is to prevent the occurrence of all kinds of fermentation. The germ theory of infective disease is, I say, an independent view, and not part of those principles at all. It is from mixing up these two theories that the confusion, and much of the difficulty in accepting the principles of antiseptic surgery, have arisen. Thus, for instance, a surgeon who writes a good deal on this subject, after admitting to the full the germ theory of putrefaction, states that he refuses to accept the principles of antiseptic surgery and aseptic surgery in its train! Speaking of the principles of antiseptic surgery, he says: 'Granting that the same germs which would inevitably produce putrefaction in a dead infusion of beef are constantly admitted to wounds, there is not the slightest particle of evidence that they do produce any change whatever upon *living* tissues, still less is there any evidence that the changes which occur in the numerous varieties of what we call blood poisonings, even when they are of an undoubtedly local origin, have the slightest analogy to those seen in a putrefying dead infusion.'

Such a passage simply shows that the author is confusing

together the two germ theories—that of putrefaction with that of infective disease. But, as I have already pointed out, *the germ theory of putrefaction is the only view at the basis of antiseptic surgery*; and the author admits the truth of this theory, and yet rejects aseptic surgery!

But let the germ theory of putrefaction be once admitted, antiseptic surgery and, where possible, aseptic surgery, is the logical, practical outcome. What does it matter for the *principles* of antiseptic surgery what the precise relations are which fermentations in wounds bear to the bad symptoms after wounds? These are points to discuss in connection with the *results* obtained, but they are not the essential points to be considered in determining the adoption or rejection of *antiseptic* methods.

Leaving out of view the question of infective disease, and supposing that we deny its connection with fermentations in wounds, I would ask any surgeon who takes the above line of argument, whether he would view with satisfaction the entrance of causes of putrefaction into the peritoneal cavity? I venture to say that he would not, and that the following would be the result of his calmer cogitations: 'I do not care at all what relations fermentations in wounds bear to infective diseases, but I would rather not have putrefaction present in the abdominal cavity, and I must insist on your taking all possible precautions to prevent the entrance of its causes. In other words, I believe in the germ theory of putrefaction, and I wish the methods of practice which follow logically from such belief to be fully carried out in this case; i.e. I wish the strictest aseptic precautions to be employed. If, at the same time, infective disease is also avoided, I shall be very pleased, but I do not understand the causes of it. I do understand, however, the causes of putrefaction, and I wish no precaution to be spared by which it may be avoided.'

I venture to think that, with the mass of evidence now existing, there will be very few disbelievers in the germ theory of putrefaction; and, as soon as a man accepts that view, he is bound, as a rational man, to put his belief into practice on one or other of the lines indicated here. He must do that, I say, whether he believes in the germ theory of infective disease or

not, were he indeed ignorant of the existence of such a view; though no doubt the precise amount of energy which he will bring to bear in carrying out one or other method will to a certain extent depend on the view which he takes of the dangers which may arise from fermentations in wounds, and these we shall have to consider when we come to speak of the results of the various methods.

CHAPTER XIV.

HISTORY OF ANTISEPTIC SURGERY.

Practice of the ancient writers. Attempts of the ancients to secure immediate union of wounds. Paré and Paracelsus: Delacroix: Arcæus: Progress of wound treatment in the sixteenth century. Seventeenth century: Magatus: Wiseman: Colbatch: Progress in the seventeenth century. Eighteenth century and the early part of the nineteenth: Boerhaave: Col de Villars: Heister: Bilguer: Benjamin Bell: Abernethy: John Hunter's objections to the views of Bell and Abernethy: John Bell: opinions and practice of other surgeons: Conclusions: Von Kern.

ANTISEPTIC surgery being, as we have seen, a very wide term, in endeavouring to take a philosophical view of its history and development, we must trace the development of the methods of wound treatment during the last two centuries at least. In doing so, and in order to avoid repetition when we come to consider the results of the various methods, I shall introduce into this historical part such details of the results obtained as seem to me advisable.

Without entering into details on the practice of the ancient writers, we may look on their modes of treatment as more or less directed to making the wound heal. Thus, substances were applied to *make* the flesh grow, others to *make* the growing flesh firm, and others to *make* the wound cicatrise. Amid all these attempts, the tendency of the *wound itself towards healing* was almost entirely lost sight of. It was supposed that without these applications all sorts of evil results would take place, and healing would not occur. These ideas reigned paramount for centuries; and we find them still advocated, not perhaps in such a glaring manner, up till very recent times.

Nevertheless, there were surgeons who from time to time were bold enough or had insight enough to protest against

these views. Amongst these we may mention PAUL D'EGINETA,[1] who lived probably in the seventh century. He proscribes the numerous plasters, by means of which, he says, the action of nature is choked, and he shows himself not ignorant that it is to nature herself that one must attribute the successive changes that wounds present. ROGERIUS,[2] in the thirteenth century, also protested against the numerous dressings then in vogue, and used only wine and honey as local applications.

The rule at that time was not to attempt to close the wound, even where that was possible, for it was supposed that all sorts of evil humours would be pent up, and cause constitutional affections. On the contrary, wounds were distended with tents and plugs, which were covered with all manner of ointments, and imbued with various kinds of medicaments. Attempts were, however, made from time to time to procure union. Among those who made these attempts may be mentioned BRUNO,[3] in the thirteenth century, who treated wounds differently according as they were simple incised wounds, or wounds with loss of substance. The former he closed at once, while the latter were made to suppurate, except in cases where nerves were injured, where he thought that 'putrefaction might cause spasm:' already we see the germ of the idea, which is at present gaining ground, that there is an intimate relation between tetanus and septic changes in wounds. Similar views were expressed about the same time by LANFRANC,[4] who wrote at length against the dangers of tents, and who states that the immediate union of wounds ought to be the first aim of the surgeon in all cases of simple wounds, except in the case of a bite by a mad dog.

During the next two centuries, surgeons seem to have forgotten, or not to have paid attention to, the teachings of Bruno and Lanfranc, and to have still continued the practice of endeavouring to remove morbid humours, and of altering the supposed poisonous state of the surface of the wound by numerous and varied applications. The most prominent surgeon of this time was GUY DE CHAULIAC,[5] the celebrated surgeon

[1] See Portal's *Histoire de l'Anatomie et de la Chirurgie*, 1770.
[2] *Ibid.* [3] *Ibid.* i. 178. [4] *Ibid.* i. 193.
[5] *Chirurgia Magna*, restituta a L. Jouberto, 1585.

of Montpellier. He used no less than five different ointments in the treatment of a simple wound.

At the end of the fifteenth century, or the beginning of the sixteenth century, a new writer appeared in the person of De Vigo,[1] who for a time exercised a great influence on the progress of surgery. It was he who elaborated the dogma that gun-shot wounds are poisonous, and that they must be burnt. He speaks of the contact of the air with wounds as being very hurtful, and for that reason he unites the edges early, taking care not to leave any blood clots between the cut surfaces. He also objects to the unnecessary use of tents, and only introduces into the wounds lint covered with a digestive made with turpentine. To the wound itself he applied various powders, some of them containing antiseptic substances, and these no doubt assisted to form an antiseptic crust.

In 1542 Michel Ange Blondus[2] wrote on the treatment of wounds by water. After arresting hæmorrhage, and removing foreign bodies, he applied dressings soaked in water.

From this period we begin the real history of modern wound treatment. The two men who were most influential in rescuing this department of surgery from the state into which it had fallen, and in laying the foundation for the more modern methods of treatment, were Paracelsus and Paré.

Philippe-Auréole-Théophraste-Paracelse-Bombast[3] was born in 1493 at Einsiedeln, near Zürich, and spent the early part of his life at Basle, as teacher of surgery. In his method of treating wounds, he only aimed at aiding nature. He supposes that there is a juice, distributed throughout the body, which keeps the various tissues of the body in good health, and repairs them when they are injured. The whole aim of the surgeon ought to be to prevent alterations in this liquid, resulting either from its contact with air or from other accidents. Nature alone is sufficient for this, as is seen in wounds of the lower animals, and the essential thing is not to interfere with nature. Medicaments are only of use as preserving this juice, and preventing its corruption (putrefaction). In the treatment

[1] *The Most Excellent Workes of Chirurgerye*, &c., 1543.
[2] Portal's *Histoire*, i. 381.
[3] *Opera Medica Chimica, sive Paradoxa*, 1603–5.

of wounds he employed silver wire sutures, and bathed or injected the wound with a solution of acetate of lead.

These views were adopted by Ambroise Paré (1509–1584 ?).[1] He says:—'Le chirurgien, pour la curation des playes, se doit proposer une commune indication, qui est, union des parties divisées' . . . 'Or ceste première et générale indication est parfaite par nature comme le principal agent, et par le chirurgien comme ministre de nature; et si nature n'est forte, le chirurgien ne pourra venir à sa fin prétendue.' Paré mentions a variety of topical applications, but his great aim is to keep the part at rest. 'Preserve the temper of a wound by low diet, a little wine, and rest; avoid venery, contentions, brawls, angers and other perturbations of the mind.' As is well known, he was the first to show that gunshot wounds were not poisonous. He simply enlarged these, and extracted any foreign bodies, and then applied suppurative medicines. He regarded wounds of joints as very fatal, and for the most part deadly, and here he counsels the application of Venice turpentine, and not of suppurative medicines. His views with regard to air are interesting. He looked on pure air as rather beneficial to the wound, and to the patient, but the air of sick rooms, camps, &c., is generally, as he supposed, loaded with miasms, and therefore very dangerous, and it is the miasms in the air rather than the air itself, which are the source of the danger.

It was chiefly by the writings and teachings of these two men, that the ideas that the wound must be fed, that bad humours must be removed, and that wounds cannot heal without constant meddling, gave place to the true view that nature is the only agent in the healing of wounds, and that all that we can or ought to do is to remove anything which interferes with its proper action. Paré looked more especially to the constitution. He strengthened the body as a whole, while removing, as far as he knew, any local disturbing causes. The former is, however, the point which he considered the most important.

Jean André Delacroix (1573)[2] was one of the most suc-

[1] *Œuvres Complètes, par J. F. Malgaigne*, 1840. See also *The Works of that famous Chirurgion*, translated by Thomas Johnston, 1665.

[2] Portal's *Histoire*, ii. 41.

cessful surgeons of that time, and adopted the views just related as to the powers of nature. He used antiseptic substances very largely. Thus he recommends strongly, as applications to wounds, ethereal oils and substances containing alcohol. After the bleeding had been arrested and foreign bodies removed, he washed out the wounds with some 'detergent liquid,' and then applied plasters containing chiefly pitch and oil of turpentine. His results were exceptionally good.

The bad effects of the air were much feared by WÜRTZ,[1] who, in order to prevent its access to wounds, kept the doors of the sick room closely shut while he rapidly changed his dressings.

FALLOPIUS[2] also, about the same time, from the same fear of the air, made a number of experiments on healing under a crust.

The method of treatment employed by FRANÇOIS ARCÆUS[3] (1574) was more simple than any of the others used up to that time. Having arrested bleeding, and removed foreign bodies, he washed the wound with alcohol, or with wine containing myrrh or other similar substances, and brought the edges together with sutures, leaving an opening which could be kept open, if necessary, by a piece of lint introduced into it. He then applied a balsam, which afterwards attained great celebrity, and which he describes as follows:—'Prenez térébenthine claire et baume élémi une once et demie de chacun; de la graisse d'un animal châtré, deux onces; vieille graisse de porc, une once. Faites foudre tous ces ingrédiens à un feu modéré, et vous aurez un liniment que vous ferez foudre toutes les fois que vous voudrez vous en servir. Vous en oindrez la plaie avec une plume et vous couvrirez le tout avec un emplâtre de Vigo.' He had some remarkable results by the use of this method, which really is a fair antiseptic method, and not far removed from an aseptic one.

VICARY,[4] towards the end of the same century, washed out wounds with a balsamic water, stitched them up, and covered

[1] *Practica der Wundarztney*, Basel, 1596.

[2] *Opera Omnia*, 1600–6.

[3] *A most Excellent and Compendious Method of curing Woundes in the Head and in other Parts of the Body*, translated by John Read, 1588.

[4] *The Englishman's Treasure, &c.*, 1626.

them with various balsams, and he speaks of the excellent results which he obtained in this way.

The great advance in this century was the reassertion of the part played by nature, and the abandonment of the ideas of feeding wounds or of making flesh. Here also we have the first strong recommendation to apply antiseptics to wounds, and the success of these applications, in the hands of Arcæus more especially, is vouched for by the fame which his balsam afterwards acquired. Nevertheless, as such methods were simply the result of chance, and did not arise from any glimpse of the true principles which were at work in obtaining the good results, they never became established modes of treatment. During this century, two writers more especially, Paracelsus and Würtz, looked on the admission of air to wounds as a very bad thing, while Paré went the length of ascribing the harm not so much to the air as to miasms contained in it.

Seventeenth Century.

The evil effects of air were, however, most prominently brought forward by MAGATUS[1] (1516), to whom also must be accorded the credit of having first thoroughly recognised the importance of rest in the treatment of wounds. He says that the air is charged with miasms, which infect the parts with which they come in contact. He points out as an instance of this, the rapid putrefaction of an egg if a hole is made into it. He also writes strongly in favour of perfect mechanical rest, for he thinks that movement is a frequent cause of the bad results which follow wounds. With the view of obtaining both objects, the exclusion of air and perfect rest, he recommends very infrequent changing of the dressings; indeed, he only changed them when it became absolutely necessary. He left spaces between the sutures for the escape of pus, but he objected strongly to the use of tents. He did not wipe away the pus from a wound, because he thought that it ultimately formed the cicatrix, and in the meantime protected the wound.

These infrequent dressings, owing to fear of bad effects from the contact of air with the wound, were, as we now know, the outcome of a wrong theory; and although Magatus's teaching exercised great influence down to recent times, yet in the case

[1] *De rara Medicatione Vulnerum*, 1616.

of those who have studied and acted on his views, the reaction has been so great when they have discovered how insufficient they were, that the good points in his method were for a long time likewise rejected.

During this seventeenth century, there was but little progress made. The accumulation of dressings, which had been given up to some extent at the commencement of the century, had been again introduced, chiefly as the result of Magatus's teaching on the bad effects of air. These dressings contained, however, balsams and other antiseptics of various kinds, and thus the bad effects which must have arisen from keeping putrid dressings in contact with wounds for a long time, may have been to some extent avoided.

It is interesting to note, in reference to our future consideration of the results of various methods of wound treatment, that Magatus's method by no means did away with the dangers of such injuries as incisions into joints. Thus, to quote from JAMES COOKE'S 'Marrow of Chirurgery' (1685); under 'Wounds of Joints,' he says: 'If a wound associate to a dislocation so that the joynt be bar'd and a little thrust out of the skin; in great joynts 'tis deadly, in all bad: yea, after set, there oft follows inflammation, convulsion, &c., especially if withal there be a fracture near the joynt.'

That advance was being made, and that the teachings of Magatus were not blindly followed, is evident, for example, from the writings of RICHARD WISEMAN [1] (1692). Wiseman's views on the healing of wounds and his methods of treatment were remarkably good. In uniting the edges of a wound, he says that one must not force a pledget of lint between the lips, nor use violence. Unite the parts gently and equally; then 'preserve the natural temperament of the part, that thereby agglutination may be obtained.' Agglutination is the work of nature alone. Blood is the natural glue, and hence great care must be taken that it is good. Attend therefore to diet and regimen. Support the patient's strength. Do not stop strong drinks, if the patient has been accustomed to them. Do not purge; but, if necessary, give gentle laxatives. He used as applications to wounds such remedies as turpentine, and outside this he applied

[1] *Chirurgical Treatises*, 1692.

cooling and astringent lotions. Powders of various kinds were sprinkled over the larger wounds.

At the end of the seventeenth century appeared a most remarkable series of treatises by SIR JOHN COLBATCH,[1] in which he describes results obtained by the use of a medicament, which he unfortunately does not mention, but which gave him results only comparable to the true aseptic results obtained at the present day by the use of the aseptic method. That he should have concealed the name of the substance employed cannot be too much regretted; but that nevertheless he was no ignorant quack, telling falsehoods simply to sell his wares, is shown by the position which he ultimately attained in his profession. The accuracy of his results are also attested partly by the publicity with which they were obtained, but chiefly by the clearness of the description which he gives of the progress of his cases—a progress which could not at that time have been imagined by any one who had not seen the facts. A typical aseptic course is described, while there is no doubt that he had observed vascularisation of the blood clot, and its replacement by new tissue, just as has been described in the present day by Mr. Lister.

In his preface he asserts 'that the method of chirurgery hitherto used is not the best; that all probes and tents, all digestive and suppurating medicines, all cauterizing and straight ligatures are injurious to the patients, and procrastinate their cure.' His view is, that when a wound is made the 'nutritious juice' escapes from the vessels. 'Now all the medicines used in the common methods of chirurgery are of such a nature as to relax the divided fibres so much, that they cannot contain the nutritious matter brought to them, but let it pass into the wound, where it is by the same medicines corrupted and turned into that substance we commonly call matter. Now the corruption of the nutritious juice cannot be performed without a sort of *fermentation*, and it is the fermenting particles that, fretting the fibres, cause inflammation in wounds, and *by entering into the blood, and dividing its texture, cause symptomatic fevers*, which frequently prove so fatal.' He then goes on to say that by the medicines in common use surgeons cause suppuration

[1] *Novum Lumen chirurgicum*, &c., by Sir John Colbatch, 1704.

and often mortification, 'and when nature is almost tired and oppressed, weak as she is, they leave her to do her own work.' In the cases where the cavities of the body are wounded, the patient is considered certain to die; 'for to bring a wound in the lungs, liver, guts, &c., to suppuration is to bring certain death, and to cure a wound without bringing it to suppuration, they don't pretend.' He then refers to the fact that in all wounds the patients were put on low diet, and more often than not blood was taken in large quantities, 'to the great weakening of their patients.'

As to Colbatch's method. 'Suppose a wound be made with a sword, or other cutting instrument, the length or depth of which signifies nothing, I make a solution of my *Powder* in water, for want of which, in urin; and as soon as conveniently I can—the sooner the better—I either squeeze, or with a *syringe*, if the wound be deep, inject into the wound some of the said solution; I then close the lips of the wound together, which, if wide and large, I stitch up. When I have so done, I apply a pledget of fine tow, wet in the said solution, to the wound; not letting it go into it, but only to cover the edges of it, and lie all over it. And if there chances to be any large artery cut, I hold on the pledget close with my hand till the flux of blood ceases, which will be in a small time, otherwise not' (he speaks of his medicine as a hæmostatic); 'afterwards I bind it on with a very easy ligature, then I give my tincture in wine.' In incised wounds *one* dressing, or at most *two*, were as many as were requisite, the second being applied at the end of three or four days. In wounds with loss of substance more dressings were necessary, and these were changed once in four days.

As to his results, he says:—

'First of all, my medicines never cause any pain, unless it be just when the external one is squeezed or injected into the wound, and that pain is little more than what would be caused by using spring water in the same manner.

'Secondly, there is no pain afterwards, but the parts are pliant and easy, as if not hurt at all.

'Thirdly, if there has bin great pain before, according as it has bin greater or less, so it has bin longer or shorter before taken off, but the greatest in a short time. As when a

wound had bin long received, and had other applications to it before mine, had bin much inflam'd and the parts adjacent much swell'd, and consequently the party in great pain, all these symptoms have bin totally taken off in twenty-four or forty-eight hours at furthest and many times in such cases the greatest pain taken off in a few minutes. But they have never bin the cause of pain, swelling, or inflammation, which, by the way, is a certain argument they are no caustics, as some maliciously and falsely report.

'Fourthly, there has never any such thing as a fever, let the wound be internal or external, attended any who have used my medicine soon after they have bin wounded; but several who have bin ill-managed before, and thrown into violent fevers, have in twenty-four hours, by the use of my medicines, bin totally freed from them, and that without blood-letting or abstaining from flesh or wine, the more of which they drink, so they keep within bounds, I always find them the better.

'Fifthly, in all incised wounds, where my medicines have bin soon enough used, and no other applications preceded, they are perfectly cured in a few days without suppuration, and I have frequently observed that at about four days' end, such wounds have bin filled with a *substance much like hartshorn jelly*, which I have conjectured to be young flesh, and in two or three days I have found my conjectures true, the said substance being converted into good flesh. But where wounds have bin long receiv'd and matter generated, they have bin cured in a small time, without repeating the application above once in three or four days. I have had to do with wounds long received, which have bin both deep and large, and by the common methods must have discharged a great quantity of matter, but the quantity of matter coming away after using the medicines, has bin so small, that I have good reason to conjecture it was no more than what was formed before they were applied.'

These statements correspond very much with what are now made by the supporters of the aseptic method, and they could hardly, more especially where he seems to describe organisation of blood clot, have been imagined at that time. Supposing that this powder were an antiseptic, the method employed is

practically the same as that which I have described when no spray is at hand: fill the wound with the antiseptic lotion; stitch up and apply your antiseptic dressing. Neudörfer, who also mentions Colbatch, thinks that this powder was a salicylate; probably, on account of the smell of roses to which Colbatch refers, salicylate of ammonia.

Thus in the seventeenth century but little real progress was made. The great aim was to exclude the air, and to avoid mechanical causes of unrest; the chemical causes of unrest were not at all understood. Colbatch's method seems to have been completely lost, no doubt on account of his concealment of the nature of the substance used.

Eighteenth Century and the Commencement of the Nineteenth.

AUGUSTIN BELLOSTE[1] (1700) reiterates Magatus's teaching. He fears the contact of the air, chiefly on account of the miasms which it contains, and in order to exclude these, and also in order to obtain mechanical rest, he recommends infrequent dressings. He fills his wounds with *charpie* and follows Magatus in not washing away the pus. He advocates immediate union in cases where bones are divided. 'L'expérience m'a fait voir,' says he, 'en mille occasions que quand un os est simplement découvert, tout consiste, pour en éviter l'altération, à le défendre des attaques de l'air. Pour cet effet, il faut procurer la réunion de la plaie le plus tôt qu'il sera possible, par le moyen des bandages propres et des remèdes balsamiques, sans la dilater avec les tentes et les bourdonnets; par là l'os se recouvre promptement, et on évite l'exfoliation, qui est absolument nécessaire quand on a donné le temps à l'air d'y faire ses impressions.'

In 1706 PARMANUS[2] speaks of a lotion which he uses for wounds, which 'resists putrefaction, prevents ill accidents, and takes away the inflammation and pain of the wound.' His dressings were kept constantly moist with this lotion, and changed once in two or three days.

In the same year ANEL[3] published an account of a method of

[1] *Le chirurgien d'Hôpital*, 1707. [2] *Chirurgia Curiosa*, 1706.
[3] Portal's *Histoire*, iv. 398.

evacuating abscesses by aspiration, without leaving an open wound. A similar kind of instrument for the purpose of removing blood from the thorax had been previously described by Delacroix (1573).

As we have seen, the dread which surgeons as a rule up to this time entertained of admitting air to wounds was not so much because it caused putrefaction of the discharges of wounds, but because it contained miasms which were hurtful to the patient. The connection between internal abscesses and wounds had been hardly as yet observed, and it was not till Boerhaave wrote, that this connection was recognised, and that a *putrid* discharge in a wound became a thing to be avoided.[1]

BOERHAAVE[2] (1720) pointed out the frequency of internal abscesses after certain injuries, and he ascribed this to the absorption of pus by the open orifices of the veins of the wound. Boerhaave went further, and ascribed the evil effects to the *putridity* of the pus. This is very evident in the following quotations, which Jeannel (Pyohémie) gives from Boerhaave's aphorisms: 'Si tum relinquitur (pus) diù in loco clauso, attenuatur, acre fit, putrescit, augetur, vicina consumit, erodit, mole, pondere et motu sinus fistulasque creat variis locis, varias, pessimas in intestino recto;' and also, 'Aut dissipata parte tenuiori reliquum durescens tumores duros, maxime circa glandulos creat, vel denique (pus) venis lymphaticis aut sanguiferis, per eroso osculo impressum absorbetur,' &c. Jeannel also quotes passages from Paré to show that he attributed the fever which accompanied wounds to a putrefaction of the pus.

Boerhaave's views were adopted by LE DRAN[3] and HEISTER; and, in 1741, we find the necessity of frequent dressings, in order to prevent putrefaction, strongly urged by COL DE VILLARS.[4] The latter author advises that if there be much suppuration, the dressing should be changed twice a day. He

[1] That wounds of the skull were apt to be followed by abscesses in the liver was long before noticed by Paré and others, but the significance of the fact was not understood.

[2] *Aphorisms concerning the Knowledge and Cure of Disease*, translated by Delacoste, 1715.

[3] Billroth, *Historische Studien über die Beurtheilungen der Schusswunden*, 1859.

[4] *Cours de Chirurgie*, ii. 1741.

employed compresses soaked in wine or in balsams, such as the balsam of Arcæus, or covered with an ointment consisting of equal parts of wax, turpentine, and oil of hypericum. In order to avoid the contact of the air the dressing was changed as quickly as possible.

HEISTER[1] (1753) used balsams very extensively, not because he held that they conduced to form new flesh, but chiefly because they removed everything that might hinder that process, more especially putrefaction. In order to prevent the access of air, which he considered hurtful, the surgeon was instructed not to remove the old dressings till the new were ready for application. Heister attended to the drainage of his wounds, and, if necessary, he made a counter-opening into which he introduced a piece of lint covered with some sort of balsam. The edges of the wound were brought together either by stitches or by plasters composed of various balsams.

The most important work, however, in which the action of antiseptics is especially recommended, was BILGUER'S[2] (1764). His method of treatment in all kinds of wounds consisted essentially in filling all the recesses of the wound with antiseptics, and laying over the wound a piece of lint dipped in an antiseptic solution. His method is specially described in connection with cases of mortification. He makes incisions through the dead parts, which incisions should be large and numerous, but should not touch the living part. He then squeezes out the corrupted humours, and pours in the following mixture: 'Of frankincense, mastick, sarcocolla, and myrrh finely powdered, true balsam of Peru, and genuine essential oil of cloves—equal parts; of balsam of Fioraventi, as much as may, after mixing all the ingredients over a very gentle fire, form a thin liniment.' Over this is laid some dry lint, thoroughly sprinkled with a powder composed of 'an ounce of myrrh finely powdered, half an ounce of sal-ammoniac, camphor and nitre—of each a drachm.' If necessary, fomentations may be applied outside this. These fomentations should be composed of 'a pint of lime-water, 3 ounces of camphorated spirits of wine, and $1\frac{1}{2}$ ounce

[1] *General System of Surgery*, translated 1753.

[2] *A Dissertation on the Inutility of the Amputation of the Limbs*, with notes by M. Tissot, 1764.

of sal-ammoniac.' A variety of other mixtures and methods of treatment are mentioned, but the quotations will show the essential points.

In this method the thorough application of these substances would no doubt result in complete asepticism; and the results which he obtained are very remarkable. From his experience, he sets his face strongly against amputation. He says: 'The cutting off a limb being the severest means employed in surgery for the relief of mankind—an operation which every one beholds with horror—I cannot, I imagine, better accomplish my design, or do a greater service than by demonstrating that the cases wherein amputation is necessary are much less frequent than has been hitherto supposed, and that it may even be almost totally dispensed with.'

With regard to his results he says: 'I have had under my care, during the course of the late bloody war, a great number of wounded limbs, torn and shattered by cannon and musket-balls, by the bursting of bomb-shells and grenades, by grape-shot, &c. I cured them without ever performing amputation . . . although there were bones broken and shattered, large blood-vessels divided, the flesh miserably lacerated, and limbs carried off; others in which the bones were split up as high as the articulation; all which circumstances might make us reasonably apprehend a tedious and difficult cure, too plentiful a suppuration, hæmorrhages, violent inflammation, excessive corruption, mortification, and death.' . . .

'I had at one time, during the war, in a military hospital, 6618 wounded persons, who were all treated according to my direction, and part of whom I attended myself. Of these, 5557 persons were perfectly cured, and in a condition to support all the fatigues of the service; 195 were able to do duty in garrison—what they call "half-invalids"—or to work at any trade; 213 remained incapable of any labour, civil or military, what they call "grand invalids," and 653 died.'

The 195 and 213 invalids belonged to the class of cases who had their bones bruised, broken, or shattered. After a calculation which I need not go into, he very reasonably concludes that 'much the greater part of these 408 men cured and sent to the Invalides would have died if amputation had

been performed.' He compares this with the 'prodigious number of wounded men who at the beginning of the war had their limbs taken off on account of dangerous wounds, of whom scarce one or two escaped with their lives.' He concludes thus: 'Further, if it be considered that many of those who died might have recovered, had they been taken care of anywhere else than in an hospital, where the air is very bad, and if it be called to mind at the same time, what some very eminent surgeons have observed, that two-thirds of those die who have their limbs cut off, I hope it will be readily acknowledged that my method of treating wounded limbs by saving them, is highly preferable to that of amputation.'

As a contrast to this method I may refer to that used by Percival Pott[1] (1768) in compound fractures. Where the limb is treated conservatively, he recommends immediate reduction, and then he mentions two requisites: 1. The maintenance of a proper opening for the free escape of discharges—a counter-opening being made if necessary; and 2. The prevention of inflammations in order that the wound may heal, as far as possible, by first intention. His dressing was a piece of dry lint next the wound, and outside this a 'pledget spread with a soft easy digestive.' As the result of this treatment in compound fractures, wounds of joints, &c., he thinks that the patient has the best chance by immediate amputation. Thus Bilguer, by his piles of dressings, but these acting more or less completely on aseptic principles, regards compound fracture as a much more favourable accident than does Percival Pott with his simple non-antiseptic dressings.

Towards the end of this century, the evil influences of air on wounds was brought very prominently forwards, more especially in England. About this time Pringle[2] published a work on the diseases of the army, in which he speaks of diseases arising in consequence of foul air, and narrates a series of experiments made with various antiseptic substances.

In 1784 Benjamin Bell[3] published his treatise on the theory and management of ulcers, in which he speaks very strongly

1 *Chirurgical Works*, edited by Sir James Earle, 1808.
2 *Observations on the Diseases of the Army*, third edition, 1761.
3 *Treatise on the Theory and Management of Ulcers*, 1784.

of the bad effects of air: 'The bad effects of air,' says he, 'on every species of sore are well known to every practitioner, but its pernicious influence on a newly opened abscess is often really astonishing. It first occasions a total change in the nature of the matter, from perhaps a very laudable pus to a thin ill-digested sanies, and afterwards brings on a quickness of pulse, debilitating sweats, and other symptoms of hectic fever, which for the most part, when the collection has been considerable, either carries the patient off in a short time, or terminates in a confirmed phthisis, which sooner or later proves fatal.' How air acts he does not know, but he thinks that it may stimulate the absorbents to greater absorption of pus, 'and it may likewise, by rendering the matter more putrid than before, give even to the same quantity greater activity in producing the different symptoms of hectic.' On this principle (of preventing putridity) he would account for the operation of many of the remedies commonly employed at that time in the treatment of sores. As a means of avoiding all these dangers, he recommends the introduction of a seton into an abscess, and he thinks that the progress of cases so treated is better than that of those in which the abscess is laid freely open. In hydrocele, however, he found that the use of the seton was followed by great pain and constitutional disturbance. From what has gone before it will be readily understood why such disturbance resulted, and also that, in the case of the abscess, no real benefit was derived.

Benjamin Bell also used leaden drainage tubes. In incised wounds, after bringing the edges together, he applies 'no dressings except a thin covering of soft lint to protect the parts beneath from cold, and to keep out the air.'

He is always reiterating the statement that 'nothing proves more hurtful to sores than exposure to the air;' and he directs his efforts to excluding the air by closely applied ointments, &c. He also details the disastrous results of wounds of joints, and he says that many authors advise immediate amputation in such cases. In small wounds, however, he draws down the skin and stitches it in such a position as to make a valvular opening.

He proposes a valvular incision for the removal of cartilages

from joints where the cartilages are quite loose, but where they are attached to any part of the synovial membrane, and the pain is insupportable, he would advise amputation as 'less painful as well as less hazardous.'

Similar views as to the bad effects of air on wounds were expressed by HUGH MUNRO[1] (1792); and he agrees with Bell in attributing the disastrous consequences following wounds of joints and incisions into psoas abscesses to the entrance of air and the putrefaction of the discharge.

ALEXANDER MUNRO[2] (1788) had previously written on the bad effects of air on serous sacs, but he thinks the *coldness* of the air is their chief cause.

JAMES LATTA[3] (1795) gives the teaching of the Edinburgh school, which is essentially that of Benjamin Bell. He says that the admission of air into abscesses 'is immediately attended with symptoms of putridity; the pus, which at first was white, thick and free from fœtor, becomes instantly thin, fœtid, and corrosive ; a quick pulse and hectic sweats come on, from which the patient scarcely recovers, if the collection of matter has been very large.'

In the ENCYCLOPÉDIE MÉTHODIQUE (1790) Alexander Munro's idea as to the coldness of the air being the injurious factor is upheld, but, at the same time, the impurity of the air is also brought forward, and an attempt is made to prove its influence by citing the different results of amputations in town and country.

The advocate of the bad effects of air who succeeded in turning his views to the best account was, however, ABERNETHY[4] (1793). He first points out in his 'Essay on Lumbar Abscesses' that 'whilst the condensed cellular substance which forms the cyst of an abscess remains entire, it continues free from inflammation, and the contained pus suffers no putrefaction, nor evident alteration of quality; . . . whenever the abscess is opened, either by ulceration or by the hand of the surgeon, a sudden and generally considerable inflammation extends over the whole cyst. This is followed by a copious

[1] *A Compendious System of the Theory and Practice of Modern Surgery*, 1792.
[2] *Bursæ Mucosæ*, 1788. [3] *Practical System of Surgery*, 1795.
[4] *Surgical and Physiological Essays*, 1793.

discharge of frequently fœtid pus.' Abernethy's writings are so well known, and so well worthy of perusal, that it is unnecessary for me to discuss them in detail. His principle of treatment was to reduce the size of the cavity to as small dimensions as possible by successive tappings of the abscess, and when this was done, the remaining sac was opened. This method was first carried out by drawing off the pus by means of a trocar and canula, but that was soon abandoned for a valvular incision. Several cases are recorded which clearly show the advantages of the method.

In investigating the reason why air does harm, Abernethy discusses the views as to whether it is on account of some irritating property which it possesses, or by causing putrefaction. He concludes, from observation of the phenomena seen in emphysema, and also from Astley Cooper's experiments, in which he inflated the abdomen with air without causing any harm, that the first supposition cannot be correct. Nor does he think that it is the putrid matter which does the harm, for 'if the matter had only an incomplete discharge, if it was confined in a state of putrefaction, and thus applied to the surface of the abscess, it surely would be in some degree injurious, but as the outlet in general is sufficient, and as the former matter is washed away by that which is newly secreted, this is not likely to be a common occurrence.' His belief is that it is partly the primary injury to the cyst when it is opened, but chiefly the *constant* action of the air. 'The circumstances, however,' he says, 'are different' (from those in Astley Cooper's experiments). 'When the opening is permanent, a constant renewal of air is permitted; and the application of a matter so unusual to these surfaces, I am inclined to believe, does harm.'

In his 'Lectures on the Theory and Practice of Surgery' (1830), he speaks of the objects to be aimed at in the treatment of wounds, viz. position, accurate union, avoidance of movement or tension, and prevention and mitigation of inflammation. Stitches are bad, and he uses plasters instead. He approves of leaving wounds open till they become glazed, and then, when the edges are brought together, he covers them very lightly or leaves them quite exposed.

The dangers of air, and the advantages of the valvular

method, were accepted by the leading German writers of this period.

JOHANN CHRISTIAN ANTON THEDEN [1] (1795) used a lotion for wounds, consisting essentially of sorrel water and alcohol. He speaks of washing out wounds with warm wine, then bringing the edges together, and applying compresses soaked and kept moistened in this lotion. He does not regard wounds of joints as so dangerous as other writers have made them out to be; and he mentions a case of compound fracture of the olecranon from a sword cut, in which there was but little fever and discharge, and the joint was ultimately only somewhat stiff. Three cases of removal of loose cartilages from the knee-joint are narrated. Pressure was made during the operation so as to prevent the entrance of air, and then his lotion was applied. Two cases did well, the third patient got a 'malignioses Fieber' after the operation, and died. Theden does not think that this bad result was a consequence of the operation.

AUGUST GOTTLIEB RICHTER (1799) [2] advocates the free removal of pus by counter-openings if necessary. He used various digestive ointments, and rather tended to the old style of stuffing an open wound without employing any special antiseptic means. It is interesting to note that with him the question of amputation in gunshot injuries and compound fractures is brought prominently to the front. He fears an absorption of pus, if attempts are made to save the limb, and this absorption is, according to him, always followed by death. He advocates Abernethy's method of opening lumbar and psoas abscesses.

These views as to the bad effects of air on wounds and in abscess cavities were not allowed to pass unchallenged, and were especially objected to by John Hunter and John Bell.

JOHN HUNTER (1792) [3] ascribes the bad consequences following a wound to the injury itself, and not to the action of the air. A disposition in the part to inflammatory action may also be superadded. In speaking of the view that air is a cause of suppuration, he says: 'Various have been the opinions on this

[1] *Neue Bemerkungen und Erfahrungen zur Bereicherung der Wundarzneikunst*, 1795.

[2] *Anfangs-gründe der Wundarzneikunst*, 1799.

[3] *John Hunter's Works*, Vol. III., edited by F. Palmer, 1835.

subject' (the question of inflammation in wounds); 'and as every violence committed from without, under the circumstances before mentioned, is exposed more or less to the surrounding air, the application of this matter to internal surfaces has generally been assigned as a cause of this inflammation; but air certainly has not the least effect upon those parts, for a stimulus would arise from a wound were it even contained in a vacuum. Nor does the air get to the parts that form circumscribed abscesses, so as to be a cause of their formation; and yet they as readily suppurate in consequence of inflammation as exposed surfaces.

'Further, in many cases of emphysema, where the air is diffused over the whole body, we have no such effect—and this air not the purest—excepting there is produced an exposure or imperfection of some internal surface for this air to make its escape by, and then this part inflames. Nay, as a stronger proof, and of the same kind with the former, that it is not the admission of air which makes parts fall into inflammation, we find that the cells in the soft parts of birds, and many of the cells and canals of the bones of the same tribe of animals, which communicate with the lung, and at all times have more or less air in them, never inflame; but if these cells are exposed in an unnatural way, by being wounded, &c., then the stimulus of imperfection is given, and the cells inflame, and unite, if allowed; but if prevented they then suppurate, granulate, &c.

'The same observation is applicable to a wound made into the cavity of the abdomen of a fowl, for there the wound inflames and unites to the intestines to make it a perfect cavity again; but if this union is not allowed to take place, then more or less of the abdomen will inflame and suppurate.

'If it was necessary that air should be admitted in order for suppuration to take place, we should not readily account for suppuration taking place in the nose from a cold, as that part is not more under the influence of air at one time than at another; nor is the urethra in a gonorrhœa affected by the air more at that time than at any other; these parts being at all times under the same circumstances with respect to air. Therefore, there must be another cause.' Truly there is another cause, as has been

already demonstrated, but not the one John Hunter supposed, as we shall see when we consider the results of subcutaneous injuries and subcutaneous operations.

John Hunter's treatment of wounds is very simple. He wished to allow nature to perform her work herself, simply applying poultices or ointments to allow the protecting dressing to come off easily.

It is in his views on healing by scabbing that we are mainly interested. He observed that when blood dried on a wound, that wound often healed without suppuration; and hence he concluded that a scab was an obstacle to suppuration, chiefly because it precludes the necessity for the formation of discharge to act as a covering for the exposed surfaces. He considered that it was the best practice to let superficial wounds scab over. Many deep-seated wounds also, where the deep parts are in contact, may be allowed to scab. This ought likewise to be done in cases of compound fracture with a small external wound. In large wounds this formation of a scab did not always succeed, but he did not think that there was any danger in trying to get it. In such cases the crust formation could be aided by the sprinkling of powders over the surface, such as chalk or lapis calaminaris. Where suppuration occurred under the crust, he did not even then remove it in the first instance, but pressed out the pus in the hope that the remainder might dry up. When, however, it was evident that harm was being done, he applied poultices and removed the crusts.

John Bell [1] likewise denied that the bad results of wounds were due to the admission of air. In the edition of his 'Principles of Surgery,' edited in 1826 by Charles Bell, he opposes strongly the idea that air can in any way cause inflammation, and he criticises severely Munro's book on the 'Bursæ Mucosæ.' He points out that in the case of abdominal wounds or of psoas abscesses air cannot enter the cavity. He further adds: 'That the air which we breathe, and which we feel upon the surface so bland and delightful, should have so opposite a relation to the internal parts, that it should there be a stimulus more acrid and more dangerous than the urine or bile, is not to be believed upon slight grounds: this misfortune of inflammation

[1] *Principles of Surgery*, edited by Sir Charles Bell, 1826.

running so quickly round all the surfaces of shut sacs, where-ever they happen to be wounded, proceeds altogether from another source, simple and plain to the last degree. For, in the wound of any shut cavity, where the parts do not adhere, the inflammation spreads and runs its course by a law of the animal economy, which we explain very ill when we call adhesion, the adhesive stage of inflammation, representing, as the first stage of a most dangerous disease, that adhesion which is a natural and healthy action, the most natural in all the system, and the farthest from disease. Thus, in a wound of the breast or belly, in a joint, or in any shut sac, if the parts, being neatly laid together, should once adhere, then there is no swelling, no pain, no formation of matter, the parts are well and sound in the very moment in which they adhere; thus it is sometimes in narrow or slanting wounds. But if the wound be broad and open, or if the least thing keeps the lips apart from each other, or if they run into inflammation, then the lips turn away from each other, matter forms, the wound inflames, and not the wound only, but also the wounded cavity inflames, so that if it be in a vital part, the man dies.'

In speaking of compound fractures and dislocations, after citing the opinion of French surgeons, chiefly Palfin and Duverney, that amputation should always be performed, he says: 'We do not comply with any such barbarous rule; . . . we know that nature will do wonders, but they are wonders, and we never enter upon the attempt of preserving a limb thus desperately fractured without awful hesitation, and when we do venture to dilate the wound, and push back the bones, or saw them off, we feel all the responsibility of what we have just done.'

Of wounds of joints he similarly says:[1] 'We here pronounce the opinion which we have too often to deliver in common practice, that openings into inflamed joints are fatal; and though there are in every book cases of anchylosed joints, we cannot forget that for one that has escaped by anchylosis, thousands have died.' Such are the results of his treatment, founded on the views we have quoted.

He says, with regard to the methods of treatment: 'When a modern surgeon allows himself to talk about the mundifying,

[1] *Discourses on the Nature and Cure of Wounds*, 1812.

incarning, and cicatrising of wounds, or directs how to fill the wound up with good and sound flesh and keep it to a fair and even level with the adjacent skin, he but proclaims his own ignorance of the properties of the living body.' What we have to do, is to 'save the patient from immediate bleeding, and to lay the wounded parts so cleanly, so neatly, and so evenly in contact with each other, that they may adhere. The rest we leave to nature.'

Sir Charles Bell,[1] although imbued with his brother's teaching, recommends the valvular method in removing loose cartilages from the joints, and states that where the cartilage has escaped into the joint during the operation, the consequences are generally disastrous, on account of the exposure of the joint.

Such is a short abstract of the views held by the greatest surgeons of our country as to the effects of air on wounds; but whatever conclusions were come to, the good results of Abernethy's valvular method were so evident that it was generally recommended. As we have seen, Richter and Theden, and, I may also say, Arnemann,[2] adopted and advised it; and Samuel Cooper[3] (1807) says of it: 'I must consider it in the present state of surgery as the only one warrantable.' Cooper also recommends a valvular incision for the removal of loose cartilages. He does not think that the situation of the incision, a point on which great stress was laid by some at that time, is of any consequence, but he makes it in a valvular manner, brings the edges of the wound accurately together, and keeps the limb extended, and completely motionless. He considers that the dangers of such operations have been much exaggerated, 'but, making every allowance for the influence of prejudice, a man must be very sceptical indeed who does not consider the wound of a large joint like that of the knee attended with real cause for the apprehension of danger.'

In 1808 John Pearson[4] wrote against Abernethy's method of opening psoas abscesses, and in favour of allowing them to burst. He says: 'The instances of those who perfectly recover from the empyema psoadicum are few in number when com-

[1] *Principles of Surgery*, edited by Sir Charles Bell, 1826.

[2] *System der Chirurgie*, 1798. [3] *Treatise on the Diseases of Joints*, 1807.

[4] *Principles of Surgery*, 1808.

pared with those to whom it proves fatal.' That his results were not so good as those obtained by Abernethy's method is evident from his description of the course of psoas abscess. He says: 'Whether the abscess be opened artificially, or be permitted to open by a spontaneous rupture, a very large quantity of purulent matter, of the density of good pus, but often inclining to a cineritious colour, is generally evacuated from its cavity. The daily discharge of pus also greatly exceeds the quantity that might be expected from a tumour of that apparent magnitude. The sore frequently puts on a scrofulous aspect, all the hectical symptoms increase, and the patient is gradually destroyed.'

LEVEILLE [1] (1812) advocated the direct incision for removal of loose cartilages.

In America WM. GIBSON [2] (1824) advised valvular incisions in operations for removing loose cartilages from joints. He recognised, however, that 'wounds of the larger joints are among the most dangerous accidents in surgery;' and the same may be said of those 'wounds of the smaller articulations, trivial in the eyes of the surgeon, but, in defiance of all calculations, sometimes followed by tremendous symptoms, and even death.'

SIR ASTLEY COOPER [3] (1819 and 1827) does not seem to have expressed any definite opinion on the effects of air on wounds. His method of dressing consisted in applying a piece of lint dipped in blood along the line of incision. This was fixed by strapping. A cooling lotion was used if there was much inflammation. I may mention here his views on wounds of joints. He advocates immediate and close union of the wound in the skin. Then he applies lint dipped in blood, and over this strapping. He covers the knee with linen soaked in a solution of acetate of lead and spirit and places the limb on a splint. As instances of improper treatment, he says: [4] 'If the patient has a poultice applied, or if the utmost attention be not paid to the immediate closure of the wound, inflammation of the synovial membrane arises, and suppuration ensues. In young

[1] *Nouvelle Doctrine Chirurgicale*, 1812.
[2] *The Principles and Practice of Surgery*, 1824.
[3] *On Dislocations*, 1819.
[4] *Lectures on Surgery*, edited by F. Tyrrell, 1827.

and healthy constitutions, these wounds in the largest joints are recovered from, but in the aged and weak they destroy life. . . Recovery from these injuries, when inflammation has followed, is by adhesion, so as to destroy the synovial surface, or else by granulation, when a partial or general ossific anchylosis is the result.'

LARREY [1] (1829) does not believe that it is so much the penetration of the air into the joint—for that very often does not occur—as the accumulation of blood and consequent tension, which give rise to the bad symptoms.

BOYER [2] ascribes the bad results of wounds to the action of air on them; but he also considers that putrid pus is a bad application. His method of treatment was accordingly to apply masses of *charpie* over the wound in the first instance, and to leave this dressing on for several days. In this way he excluded the air till granulations had formed, and he looked on them as sufficient protection of the wound against the influences of the air. He therefore afterwards changed the dressings frequently, in order to remove the putrid pus.

I need not go over his results in compound dislocations, wounds of joints, &c. So far as they are given, they do not differ essentially from the results of others. Thus six cases of wounds of joints are detailed, of which four died, and Phil. Boyer, who edits the work, refers to ten cases, all of which ended unfavourably.

I have included a few writers of the present century along with those of last century, because they merely speak of results obtained by methods practised at that time. The whole facts as yet stated may therefore be taken as showing the state of surgery up to the year 1809.

Let us now methodise the results as yet obtained from an antiseptic point of view.

The bad effects of the air, down to Priestley's discovery, were generally supposed to be due to the temperature of the air. Paré and others had, however, as we have seen, added to this view the further supposition that it carried miasms to the

[1] *Clinique Chirurgicale*, 1829.
[2] *Traité des Maladies Chirurgicales*, &c., edited by P. Boyer, 1844.

wound; while Benjamin Bell first spoke of the bad effects of the gases, more especially of the 'fixed air.' We shall see that this latter view has been more developed in recent times.

Others, looking on the putrefaction of the discharges as a potent source of evil, attempted to prevent this by the application of various balsams and other antiseptics; and, in two instances (Colbatch and Bilguer), with very great success.

Many surgeons, however, saw in the better results of their time merely the effect of simplification of dressings, and, acting on this idea, they reduced their dressings to a minimum. Among those who held this view, and who have not been mentioned, were LOMBARD [1] and PERCY,[2] who, in 1785, learned from an Alsatian that he had an infallible remedy for wounds. This turned out to be river water used along with certain magic utterances. Percy and Lombard employed water henceforth in various ways, and became enthusiastic in its praise.

In 1809 VINCENZ VON KERN published a little book entitled 'Avis aux Chirurgiens, pour les engager à adopter une méthode plus simple, plus naturelle, et moins dispendieuse dans le pansement des blessés.' In his method the wound was washed with tepid water, left open for eight to ten hours, then united with strips of plaster and covered with light compresses dipped in tepid water. To provide a drain the ligatures were all brought out at one part, or else a piece of oiled lint was introduced at one of the angles. The wound was cleansed once or twice daily by washing it with tepid water. In some cases he applied poultices. He says: 'Cold water for arrest of hæmorrhage, then warm water for the dressing, some small pieces of lint, absolute rest, and artificial heat: see! that is all that is necessary for the treatment of any sort of wound.'

Von Kern held, that the ordinary methods of dressing heated the wound and favoured inflammation and suppuration; that they irritated it mechanically and chemically and, in the case of stumps, by their weight, caused retraction of the soft parts. He considers air as not only not hurtful, but in fact useful. 'Folget meinem Beispiele:' he exclaims. 'Durch

[1] *Clinique Chirurgicale relative aux Plaies*, 1798.

[2] *Manuel du Chirurgien d'Armée*, 1792. See also *Opuscules de Médecine*, &c., 1827.

Anwendung dieser Grundsätze werdet ihr den Kriegern unendliche Schmerzen und dem Staate Millionen ersparen.'

Von Kern's method, which was essentially water dressing —a septic dressing—and which I mention chiefly as a matter of history, was adopted by VON WALTHER in Bonn, and by FRITZE in Prague. It was brought to England more especially by LISTON, and up till 1860 was pretty generally adopted in this country.

It did not spread much in France; and here is ROCHARD'S[1] explanation, which is well worthy of careful attention: 'Si ce mode de traitement, si rationnel et si économique, n'a pas pu se généraliser en France, cela tient surtout aux conditions hygiéniques des hôpitaux de nos grandes villes. Les Anglais, plus favorisés que nous, ne voient pas l'infection purulente incessamment suspendue sur la tête de leurs malades, et c'est cette menace qui a de tout temps préoccupé les chirurgiens de Paris. *Les pansements à l'eau ne leur ont pas offert contre elle une garantie suffisante; il fallait des préservatifs plus certains,*[2] ou qui du moins parussent l'être, et à l'époque à laquelle nous nous reportons (1860) ils se livraient à cette recherche avec une ardeur et une fécondité d'imagination des plus louables.'

In the further history of this subject we must, up till quite recent times, confine our attention to the progress of wound treatment in other countries. In England, where better hygienic conditions prevailed, this subject was almost entirely neglected; and the chief aim of the surgeon was to perfect the methods and instruments for operating, and to attain great speed and dexterity in the performance of operations.

[1] *Histoire de la Chirurgie Française*, 1875. [2] The italics are mine.

CHAPTER XV.

HISTORY OF ANTISEPTIC SURGERY—(*continued*).

History of the various methods. Incubation: Guyot. Subcutaneous surgery, preliminary attempts: Stromeyer: Dieffenbach: Jules Guérin: Langenbeck: Other authors. Occlusion: Jules Guérin: Chassaignac—Rochard's remarks: Pansement ouaté—Alphonse Guérin, method and results—Ollier. Substitution of various gases for air: Demarquay and Leconte. Open Method: Bartscher and Vezin: Burow: Humphrey. Healing by scabbing: John Hunter: Neudörfer: Bennion: Lister: Bouisson: Bonnet, etc. Irrigation and the water-bath: early history: Jossé: Bérard: Mayor: Amussat: Langenbeck: Valette.

We must now trace the different modes of treatment to which the ideas as to the cause of the bad effects which often follow wounds have given rise.

Incubation.

As has been already mentioned, the view for a long time was that it was the cooling and drying effect of the air on the wound which had to be guarded against. Since Priestley's discovery this idea has been more or less abandoned; but in 1835 and later, M. Jules Guyot [1] studied the effects of cold, and attempted to found a method of treatment on his views. Guyot adduces evidence from Paré and Larrey to shew that wounds cicatrise most rapidly in warm air. Larrey, in his 'Campagne d'Égypte,' states that the wounds in that hot climate cicatrise with astonishing rapidity; and in his 'Campagne d'Allemagne' he makes the opposite remark as to the deleterious effects of cold. Guyot accordingly made a series of experiments on animals, and found that when wounds were kept at a temperature

[1] *Archives Générales de Médecine*, Vol. VIII. 1835. See also *De l'Incubation et de son Influence thérapeutique*, Paris, 1840; and *De la Chaleur dans le Traitement des Plaies*, 1842.

of about 35° C., they healed with great facility, and much more rapidly than similar wounds left exposed to the ordinary temperature.

He applied this method to wounds in man. His object was to surround the part with a uniform and sufficiently elevated temperature (about 36° C., and not below 28°–30° C.) He enclosed the wounded part, without any dressing, in a box, into which a current of warmed air was constantly introduced through a pipe. The box had glass sides, so that the wound could be always seen. This treatment was continued for from ten to twenty days. At first there was a very abundant serous discharge and by-and-by pus. The pus dried up, forming crusts, which were removed every two or three days. Guyot says that wounds thus treated heal more rapidly than by any other method, and also that wounds, such as some forms of ulcers, which refused to heal at the ordinary temperature, healed readily at a temperature of 36° C.

This method only merits the term antiseptic to a limited extent. No doubt the heat made the discharge more concentrated, and possibly unfit for the growth of organisms, while at the same time mechanical rest was obtained. Nevertheless, the method did not fulfil the expectations of its introducer, and, partly for that reason, and partly also because it was so unwieldy, it has been completely abandoned.

Subcutaneous Surgery.

A much more important outcome of the idea of the bad effects of the gases of the air was, however, the introduction of the SUBCUTANEOUS method—a method which has maintained its place up to the present time and will probably always continue to do so to a certain extent.

Already in the last century, and indeed earlier, the foundation of this method had been laid. DELACROIX and ANEL, by their methods of aspirating cavities containing blood or pus, and more especially ABERNETHY, by his valvular incisions into abscesses and into joints, had carried out the principle more or less completely. Nevertheless it is to the introduction and practice of subcutaneous tenotomy that we owe the spread of the subcutaneous method.

In 1807, Sir CHARLES BELL advised the subcutaneous division of the ligaments by means of a cataract needle in cases of irreducible dislocation of the thumb. There seems to be no evidence, however, that he ever put his suggestion into practice.

The first real attempt at conducting an operation under the skin was made by DELPECH in 1816.[1] Delpech, in spite of what has been thought and said to the contrary, performed the operation of tenotomy in this way in order to avoid the contact of the air with the divided tendon. Of the air he says: 'Cet agent était au moins un stimulant de plus qu'il paraissait prudent d'éviter; *c'est dans ce dessein que nous avons pratiqué notre opération*, de manière à ne point intéresser la peau qui recouvre le tendon.' The old method of dividing tendons was to make a longitudinal incision over the tendon, expose and divide it. Delpech made a small incision on each side, introduced a narrow knife, and divided the tendon without exposing it. Unfortunately suppuration occurred in his first case.

Probably in this same year BRANSBY COOPER divided the tendo Achillis subcutaneously.

In 1817, BENJAMIN BRODIE applied the same principle to a case of varicose veins, which he divided subcutaneously. This operation was at a later period revived by RICORD.

ASTLEY COOPER, probably looking at division of tendons from the old point of view, objected to subcutaneous tenotomy, but he recommended subcutaneous division of contracted palmar or plantar fascia, and in 1822 BRANSBY COOPER performed such an operation. Neither surgeon makes any remark about the principle on which these operations ought to be performed.

In the same year (1822) DUPUYTREN performed the first subcutaneous myotomy. The muscle operated on was the sterno-mastoid. The account of the operation is not published, so far as I can find, by himself, and the accounts given in various works differ much. It seems, however, to have been a case of contracted sterno-mastoid in a female, and Dupuytren divided the muscle subcutaneously, not from any idea of excluding the air, but simply with the view of avoiding a large scar. Indeed his incision seems to have been more than an inch long, so that only part of the operation was done

[1] *De l'Orthomorphie*, &c., 1828–29.

subcutaneously, and it was not at all performed on the subcutaneous principle.

In 1830 Dieffenbach[1] speaks of Dupuytren's operation as having been frequently performed with success, and Syme, among others, repeated it in 1832 strictly subcutaneously.

The various attempts at subcutaneous surgery which I have mentioned remained, however, in the main fruitless, till Stromeyer, and after him Dieffenbach, took up the subject.

Stromeyer seems to have performed his first operation in 1831, and his first publication was made in 1833.[2] He only operated on tendons. The object of his operation is distinctly stated to be the exclusion of the air by making as small an incision in the skin as possible. He looked on suppuration and sloughing of the tendon as the consequences were air admitted. He improved Delpech's operation by omitting one of the incisions, simply making a single incision of sufficient size to permit the introduction of a narrow-bladed knife. In his 'Beiträge zur operativen Orthopädie,' published in 1838, he narrates a great number of cases of division of various tendons throughout the body.

Dieffenbach, who had been performing Dupuytren's operation pretty extensively, no sooner heard of Stromeyer's results, than he at once adopted the practice, and his publication in the 'Archives générales de Médecine,' in 1835, narrating numerous cases, excited the greatest interest, and along with Stromeyer's results firmly established the method.

The most important writer on the subject, though in no way possessing any claim as its originator, was undoubtedly Jules Guérin. Though, as I have said, not possessing any claim as originator of the method, he was the first to study, and describe accurately, so far as the state of science at that time permitted, the principles on which subcutaneous surgery was based, and thus he paved the way for the more general application of these principles. In his 'Méthode souscutanée,' published in 1841, he describes his views in detail, and gives a number of results. His first operations were performed in 1836, and in these he remarked the constant absence of in-

[1] See also *Ueber die Durchschneidung der Sehnen und Muskeln*, 1841.
[2] *Ueber Durchschneidung der Achilles Sehne. Rust's Magazin*, Bd. 39.

flammation, and the rapid organisation of the wound. Comparing his results in this respect with the details given by Dieffenbach, Lisfranc and others, in which the frequent occurrence of inflammation and abscess is mentioned, he came to recognise the principles of subcutaneous surgery to the full, or at least so far as they could be recognised in the then state of science. He was thus led to establish the following law: 'That all wounds made subcutaneously, whatever be their seat and the nature of the tissues divided, possess the property of subcutaneous injuries of tendons, that is to say, do not inflame nor suppurate, but undergo immediate organisation.' Both on man and animals he found that the most extensive wounds, such as division of the great mass of the dorsal muscles, were not followed by any trace of inflammatory symptoms.

He indicates some of the applications to which this principle of making the wound under the skin may be applied. Of these the following are the most important: Incisions into serous pouches; subcutaneous incision of commencing inflammatory swellings; subcutaneous removal of exostoses, leaving the detached portion to be absorbed, or to be removed after healing of the bone; opening chronic abscesses; numerous myotomies and tenotomies of all kinds.

About this time various operations other than tenotomy were performed subcutaneously. Thus Barthélemy, Malgaigne and Velpeau (the two former with success) opened ganglia subcutaneously. M. Ricord, as we have mentioned, operated subcutaneously on the veins in varicocele. He preferred to ligature them.

Dufresne Chassaigne, and about the same time, or somewhat later, Goyraud and Syme, proposed and carried into effect a subcutaneous method of removing loose cartilages from the knee-joint, by dividing the capsule subcutaneously, expelling the cartilage, and leaving it in a bed in the cellular tissue, from which it could be extracted at a later period.

In England, William Adams [1] published a pamphlet on subcutaneous surgery in 1857, in which he shews himself a strong advocate of the subcutaneous method. In support of the generally accepted views as to the principles of subcutaneous

[1] *Subcutaneous Surgery*, 1857.

surgery, he quotes the following passage from Paget: 'For of the two injuries inflicted on a wound, the mechanical disturbance of the parts, and the exposure to the air of those that were covered, the exposure, if continued, is the worst. Both are apt to excite inflammation; but the exposure excites it most certainly, and in the worst form, i.e., in the form which most delays the process of repair, and which is most apt to endanger life.'

As pointed out by Dr. Henry Dick,[1] Jules Guérin, by his operation for removing exostoses, must be looked on as the first to operate subcutaneously on bones. Langenbeck, however, is the man who has popularised subcutaneous osteotomy. The idea first occurred to him during the Schleswig-Holstein war, in 1848, where he had introduced small straight pointed saws for section of bones. His first operations were not strictly subcutaneous, but in 1852 he practised subcutaneous operations for anchylosis of the knee, and he soon extended his method to the division of rickety bones. Meyer seems to have operated more perfectly for rickety deformities. Gross, in 1859, performed osteotomy for deformity of the femur. This operation was followed by abscess at the seat of fracture, but the patient made a good recovery.

In 1869 Adams[2] extended this principle to division of the neck of the femur for anchylosis of the hip-joint.

Quite recently Ogston has divided the internal condyle of the femur subcutaneously in cases of genu valgum. Ogston however always operates with strict Listerian precautions, but other surgeons still practise the operation subcutaneously, without bad result.

Occlusion.

Attempts have been made at various times to apply the supposed subcutaneous principle of the exclusion of gases in the treatment of wounds, not in the first instance subcutaneous. Thus have been produced the various methods of treatment by *occlusion*. We have already become acquainted with the at-

[1] Adams' *Subcutaneous Surgery*.
[2] *A New Operation for Bony Anchylosis of the Hip-joint*, 1871

tempts of the ancients to exclude the air, by applying masses of dressings. I do not refer to these, but to the more recent attempts which have been made since the principles of subcutaneous surgery have been discussed.

In 1839, in a memoir communicated to the Academy on the subject of subcutaneous surgery, JULES GUÉRIN ascribed its good results to the exclusion of the air from the wound. He further enunciates as a proposition: 'Que les applications du phénomène de l'organisation immédiate des plaies souscutanées sont de ramener toutes les plaies avec libre communication à l'air aux conditions des plaies souscutanées.'

From that time he tried various means for the purpose of converting open wounds into subcutaneous ones, such as the application of goldbeater's skin, caoutchouc, &c.; of these he considers, in 1844,[1] that goldbeater's skin gave the best results. In 1844 LAUGIER also described a similar method of treatment in a paper entitled 'Sur l'heureux emploi du mucilage de gomme arabique et de la baudruche dans le traitement des plaies suppurantes.'

CHASSAIGNAC[2] also brought forward an identical method, and claimed priority over Jules Guérin. Chassaignac used the method in abscesses, as well as in wounds. He held that the walls of abscesses, whether acute or chronic, resembled the surface of a recent wound, and by opening them by a small puncture he hoped to get adhesion of the walls. He covered his puncture with diachylon plaster. (There is here nothing essentially different from Abernethy's method.) Five years later Chassaignac said that this method had been so successful that he had never observed a single case of erysipelas, of phlegmon, or of hospital gangrene, even in the most unhealthy hospitals, under this method. Nevertheless, when he began to work at drainage he readily abandoned occlusion.

Rochard's[3] remarks on Chassaignac's statement are very much to the point, and well worth quoting: 'Ce n'est pas sans quelque étonnement qu'on voit se produire de pareilles assertions à chaque nouvelle méthode qui apparaît. En les prenant au pied de la lettre, on serait forcé d'en conclure que

[1] *Gazette Médicale*, 1844. [2] *Annales de Thérapeutique*, 1844.
[3] *Histoire de la Chirurgie Française*, 1875.

les accidents consécutifs des plaies doivent être à peu près inconnus dans les hôpitaux de Paris, puisque tous les chirurgiens qui y pratiquent se flattent de les éviter sûrement par la méthode qu'ils ont adoptée; mais on ne sait que trop bien à quoi s'en tenir sur ce point, et quant à ce qui concerne les cuirasses de diachylon, Broca rappela qu'il avait vu mourir d'érysipèle à Lariboisière, et dans le service même de l'inventeur, une femme qui avait été pansée de cette manière, à la suite d'une ablation du sein. Chassaignac, du reste, ne tarda pas à modifier son traitement en y introduisant un élément nouveau, qui absorbe bientôt tout le reste: on comprend que nous voulons parler du drainage chirurgical.'

Collodion was introduced in America in 1848, as an application to the surface of the line of incision.

In 1866,[1] Jules Guérin developed his ideas further, and described a complicated apparatus for the purpose, not only of excluding the air, but also of removing the discharge. He, unlike Chassaignac, had failed in obtaining any very satisfactory results from the methods formerly described, and he thought that this was because, though the impermeable material was closely applied in the first instance, it soon became lifted up by the discharges from the wound, and thus air got in. Then he also feared that by that method there would be accumulations of discharge, and that if these became putrid, their presence would be worse than that of air. I need not enter into a description of his method, which consisted essentially in applying an apparatus fitting the limb closely, and from which the air was pumped out. In this way none of the gases of the air were in contact with the wound, while the discharges flowed freely out. He stated that in cases so treated there had been no inflammation, or the inflammation had been shortened and reduced in severity.

An absolutely identical method was brought forward in 1867[2] by Maisonneuve, as something quite new. The only difference was, however, that, instead of pumping out the air once for all, he was continually pumping it out, and he called this 'L'aspiration continue.'

[1] *Gazette Médicale.* [2] *Comptes-rendus*, Vol. LXV. 1867.

Lannelongue also published a method, in which he used a double-walled india rubber covering, and pumped air into the space between its walls. The inner sac, applying itself closely to the limb and the wound, prevented any air from coming in contact with the latter.

There seems to be no doubt that whatever credit accrues to any one for the introduction of these methods is due to Jules Guérin; and as the logical development of the view that the noxious agents in the air are the gases of the air, these attempts, with their failures, are of great interest and importance.

In spite of the glowing terms in which Jules Guérin spoke of his method, it was not found to be of any use in the hands of others, while, what is of more importance, it broke down in the hands of Guérin himself during the siege of Paris. In a discussion at the Academy of Medicine of Paris, in 1875, on Alphonse Guérin's cotton-wool dressing, Jules Guérin was asked about his results during the siege. He stated that at an ambulance (l'ambulance des ponts et chaussées), to which he was attached, he had treated only wounds by his method, but wounds which would otherwise have led to amputation. He said that his success was great, and that Nélaton, hearing of it, asked him to apply his method to his (Nélaton's) amputation cases. He tried it in three cases, all of which died, just as the amputation cases treated otherwise did. Jules Guérin then says: 'Cette effrayante mortalité avait sa raison dans l'infection générale de l'hôtel, qu'il était impossible d'aborder sans en être averti par une odeur nauséabonde. . . . Je me borne à dire qu'en présence de telles conditions j'ai refusé d'étendre au-delà de ces trois sujets l'application de mes appareils au traitement des amputations pratiquées dans un milieu aussi profondément infecté.' These remarks are very interesting, as showing how useless this method was to protect against such accidents in situations favourable to their development, and also as showing how very little confidence M. Jules Guérin himself had in it when employed under unfavourable conditions.

A more important method of occlusion, and one which yields much better results, is the '*Pansement ouaté*,' introduced by Alphonse Guérin in 1871. We have already seen (p. 280

that this is really a method of occlusion, and not an aseptic method, as asserted by its author.

The first publication was made by HERVEY[1] in December, 1871, and several details of the method and results are there given. I shall, however, refer to this paper later, and in the meantime take some facts from BLANCHARD'S 'Thesis,'[2] published in May, 1872.

The mortality during the siege of Paris was excessively great; indeed, Hervey says that from September, 1870, till February, 1871, A. Guérin only saved one case of amputation. Guérin, who had for some time held the view that the cause of pyæmia was a miasm carried by the air to the wound, came to look on this miasm as particulate, though in 1869 he seems to have regarded it as gaseous. Acting on the view that the miasm was particulate, he applied large masses of cotton-wool in the hope of excluding it by filtration. There are two other principles on which this dressing acts, which are mentioned by Blanchard, viz., elastic compression and constant temperature.

In using this dressing, ordinary rolls of cotton-wool are employed, and cotton bandages. The cotton-wool is applied in such mass as to allow the most energetic compression without pain, say from a half to two kilogrammes of the wool. A region more extensive than the wound must be enveloped in it. The two precautions to which I have referred before must be attended to rigorously, viz. (1) the dressing must not be applied or renewed in the ward, but in an amphitheatre or room at some distance from the wards; (2) the cotton-wool ought not to be opened in the wards, because there the air is always more or less contaminated. Let us suppose that we have to deal with an amputation wound. The ligatures having been applied, are cut short; the wound is then washed with tepid water, in order to see the bleeding points, and afterwards with some antiseptic liquid such as carbolic or alcoholic water; the limb is then dried and the cotton-wool is laid over all the surface of the wound, new layers being applied till the stump is completely filled. The limb is now enveloped with rolls of cotton-wool as far as the upper part of the thigh. Then the

[1] *Pansement à l'Ouate. Archives Générales de Médecine*, 1871.

[2] *Etude sur le Pansement ouaté*, 1872.

bandage is applied, at first lightly, but afterwards more and more firmly. When it is sufficiently tight, the cotton-wool will not yield any more, and the note on percussion is tympanitic. If the bandage gets loose, apply a new one. If the discharge comes through, apply more cotton-wool outside the dressing. If the smell is very bad, wash the dressing with camphorated alcohol, or with carbolic acid, or place pieces of camphor in the bed. As a rule the first dressing is changed twenty to twenty-five days after the operation. After removing the external layers, the deeper parts are moistened with water; the wound is then washed with an antiseptic lotion, and a fresh dressing is applied.

The advantages which are claimed for this method are—suppression of pain, absence of traumatic fever, diminution of suppuration, and ease in moving the patient.

Blanchard states that during the first period, i.e. from April to the end of June, 1871, forty-one cases of amputation and resection were treated in this way, and of these seventeen died.

		Deaths
12	amputations of the thigh	6
11	,, leg	6
6	,, upper-arm . . .	1
4	,, fore-arm	1
3	disarticulations at the shoulder-joint . .	0
5	resections	3
41		17

3 of these cases died of pyæmia.
2 ,, tetanus.
1 amputation of the thigh, from secondary hæmorrhage after twenty-seven days.
1 ,, septicæmia? no metastatic abscesses;
1 ,, shock.
1 ,, an infant of two months: could not be fed.

Of the remaining eight fatal cases some were not under A. Guérin's care, and, according to Blanchard, were not well attended to. Others died of pyæmia, even after they left the hospital.

In the following period there were:—

1 amputation of the thigh	Healed.
1 ,, great toe	,,

1	amputation of the middle and first finger . .	Healed.
1	„ index	„
1	resection of a finger	„
1	„ metatarsal bone	„
1	wound of hand	„
1	„ index	„
1	„ thumb	„
1	„ extremities of middle and ring fingers .	„

(Truly a formidable series of cases to aid one in coming to a decision on the advantages of the method !)

1	compound fracture of the radius	Healed.
1	„ humerus . . .	„
1	„ olecranon with opening of the elbow-joint . .	,

Further statistics are given by Hervey :—

1	compound fracture near the knee. Resection, afterwards amputation, exhaustion	Death.
1	amputation of thigh. High temperature and general unsatisfactory progress ; injection of quinine . .	„
1	amputation of first metatarsal bone	Healed.

Under M. Panas at Saint Louis :—

1	compound comminuted fracture of both bones of the leg. Gangrene, amputation, rigors	Death.
1	amputation of fore-arm for disease of wrist. Advanced phthisis; diarrhœa before operation	„
1	amputation of the thigh for white swelling ; eight days later . .	Sudden death.
1	amputation of crushed foot . .	Healed.
1	„ lower third of leg for frost bite. Ulceration, necrosis, re-amputation at seat of election .	„
1	amputation (secondary) for crushed foot	„
1	amputation of thigh (limb torn off)	„

I give these statistics here, but I shall refer to them after-

wards. Of course, in judging them, the infected state of the atmosphere must be taken into account.

In this method no attempt was made to obtain primary union, but in 1875, at the discussion on this method of dressing at the Academy,[1] M. Guérin stated that he then stitched up his wounds before applying his dressings, and that he frequently got union by first intention. He said also, that in the Hôtel-Dieu he had been having good results, but he does not give any statistics which can be used.

At that discussion, Pasteur and others pointed out that this was not an aseptic dressing, that bacteria and their spores were present in the cotton-wool when applied, and could be readily found in the discharge. Gosselin, who opened the discussion, mentioned similar facts, and he ended by saying that the dressing was good—(1) 'parce qu'il met à l'abri de l'inflammation suppurative trop intense; (2) parce qu'il satisfait à cette indication par sa grande qualité d'être un pansement rare, qui maintient, sans interruption, l'occlusion, la protection, l'immobilité, la température uniforme, l'insensibilité, et la satisfaction morale, toutes conditions qui, si la santé antérieure n'est pas trop mauvaise, et si l'hygiène atmosphérique n'est pas trop défectueuse, conduisent à ce résultat très simple, et cependant bien grand, la formation rapide et sans étrave d'une membrane pyogénique ou granuleuse essentielle et promptement réparatrice.'

Of late Guérin has wet the deeper layers of the wool with carbolic lotion, and indeed in some cases uses a spray, for what reason it is difficult to imagine.

VERNEUIL[2] also speaks very highly of this dressing, and attributes its good results to the absolute immobility which is maintained, for by this means the granulation layer is preserved intact, and thus neither bacteria nor their products can enter the blood.

That it is not an aseptic method is evident from the whole description, and that it is not a very powerful antiseptic method is also evident. M. Verneuil, whose good results I have just alluded to, says of it: 'Quelle que soit l'épaisseur des couches entassées, le bandage, au bout de quelques jours, exhale un odeur désagréable. Le pus qui baigne la plaie est d'*une*

[1] *Bulletin de l'Académie de Médecine*, 1875. [2] *Amputations*, 1880.

extrême fétidité. Il renferme en quantité des vibrioniens et des bactéries, dont la putridité existe dans les profondeurs du pansement; et si les germes du dehors sont arrêtés au passage, ceux du dedans sont emprisonnés et forment à la blessure une atmosphère constante. Et qu'on ne dise pas que les matières putrides intérieures sont d'autre nature que les autres; car il résulte des expériences inédites de M. Poncet [1] que *le pus du bandage inoculé à des animaux a toutes les propriétés des matières septiques.*' He alludes to the following fact of extreme interest: 'Un élève de mon service, très bien portant du reste, était dans ce cas. Il était chargé du pansement d'un malade atteint d'écrasement des orteils, et qui était traité par l'ouate. *Chaque fois que l'élève renouvelait le bandage, il était pris de malaise et de diarrhée* presque subite, exactement comme lorsqu'il faisait la dissection ou l'autopsie d'un sujet putréfié.'

M. Ollier,[2] who soon saw the defects in this dressing, tried to remedy them by soaking the deeper layers of the cotton-wool in carbolic oil. He also applied a silicate bandage outside the cotton bandage. In August, 1872, M. Poncet gave the results of this modification. Of 10 patients operated on, 4 died—2 of septicæmia, and one of hospital gangrene.

In 1875, Ollier stated that he had got great benefit from this modification. He said that he had seldom seen a case of erysipelas; indeed in one winter, when it was very prevalent, only one case dressed in this way had erysipelas, while among the wounds treated otherwise he had 22 cases. He also thinks that when pyæmia appears under this dressing, it is of a mild form.

Substitution of various Gases for Air.

Other attempts were made to avoid the supposed evil effects of the gases of the air on wounds by *substituting other gases for them.* According to Rochard, these attempts were commenced in Clifton's laboratory while Humphry Davy was there. Perceval, then Ingenhousz, Beddoes, and John Ewart tried to utilise the analgesic properties of carbonic acid on ulcers. These attempts were renewed by Mojon in 1834, and by Simpson and Follin in 1856.

[1] *Lyon Méd.*, 1875. [2] *Comptes-rendus*, Vol. LXXX.

Two years later DEMARQUAY and LECONTE[1] published a paper giving the results of an elaborate investigation on the effects of injecting various gases into serous cavities, or into the cellular tissue. The gases with which they experimented were air, nitrogen, hydrogen, oxygen, and carbonic acid. They came to the conclusion that none of these gases had any hurtful action when injected into the peritoneal cavity, or into the subcutaneous cellular tissue.

As to their effects on subcutaneous wounds, they state that subcutaneous tenotomy wounds into which *air* is blown every day organise much in the same manner and after the same lapse of time as simple tenotomies. *Oxygen* alone, when introduced daily, delays matters somewhat, but it never produces the abnormal vascular conditions caused by hydrogen. *Hydrogen* retards the healing for an indefinite time; indeed healing may be incomplete even after seven and a half months. *Carbonic acid* favours in a high degree the cicatrisation and organisation of the wounds, and healing is complete in a much shorter space of time than if the wound were left to itself.

From the result of these experiments they devised an apparatus for the purpose of keeping wounds in contact only with carbonic acid gas. In 1859 they say of the results: 'Many patients affected with gangrenous ulcers, with diphtheritic wounds or with wounds in an unhealthy state, having resisted the ordinary methods of treatment, have been treated by us during more than two years in the surgical department of the "Maison municipale de Santé," and have healed with a rapidity which was truly remarkable.' Similar experiments to those of Demarquay and Leconte were performed by MALGAIGNE in 1844 with air, and he also came to the conclusion that air does not impede the healing of wounds.

At the discussion on the influence of air on wounds in 1857[2] VELPEAU denied that air as such was deleterious, and said: 'Il y a longtemps que je me suis attaché à démontrer qu'il agit alors, non à la manière d'un irritant direct, *mais en raison des décompositions chimiques qu'il provoque dans les liquides.*

[1] *Comptes-rendus*, Vol. XLIX. See also *Essai de Pneumatologie Méd.* Paris, 1866.

[2] *Bulletin de l'Académie de Médecine.*

Ce sont ces liquides, et non pas l'air, qui, altérés, dénaturés, deviennent irritants pour les tissus qu'ils touchent.'

Open method.

While these discussions on the influence of the air, and these attempts to exclude the air from wounds, were going on in France, a method of treatment, apparently of the very opposite character, was being employed in Germany with better results. I refer to the *open method* of treatment.

Though Von Kern and Walther's methods were simply the use of water dressing, yet many of their wounds were left quite open. But the first to use the open method properly so called were BARTSCHER and VEZIN. The account of their method and work is published in the 'Deutsche Klinik' for 1856 by Dr. Vezin.

Thirty years before that time (about 1826) Dr. Bartscher asked Vezin to assist him at his first amputation of the thigh. After the operation, the dressings were applied, and the patient was left. During the night, bleeding having occurred, Vezin was called. He removed the dressing, and tried to find the bleeding point, but could not do so. He therefore concluded that the hæmorrhage was due to the pressure of the dressing, and accordingly he did not apply a new dressing, but simply brought the edges of the wound together by means of strips of plaster. Next morning this plaster had slipped, and the wound was quite open. No further dressing was applied, and the wound healed well.

They soon found that wounds healed perfectly well, if left to themselves, without any interference on the part of the surgeon; and ultimately they laid down the following as the best method:—Insert no stitches, apply no dressing, simply throw over the wound a piece of linen to exclude the flies, and use a cage to keep off the bed clothes. The pillow on which the stump lies is changed daily, but no attempt is made to cleanse the wound. During the first fourteen days little progress is made. The wound becomes covered with crusts, which crack and let the pus flow out. Healing is generally complete in eight weeks. Their cases were treated in a small hospital, each patient generally having a room to himself in the first instance.

Twenty-eight cases were treated in this way, with three deaths, consisting of—

14	amputations of the thigh	. . .	No death.
6	,, leg	. . .	2 deaths. (6 and 36 days after).
4	,, upper-arm	. .	1 death. (28 days).
2	,, fore-arm	. .	No death.
2	,, great toe, metatarsal		,,

The causes of death are not given.

In 1859 BUROW [1] published his method, which differed from the other in that the wound was only left open for half an hour or so till all oozing had ceased. The edges of the wound were then brought together by means of stitches and plasters, and a piece of lint was laid over it to keep out the flies. The stump swells up enormously during the first few days, and Burow thought that this was a good thing. He says that healing is much quicker by this method than by any other. In 1866 [2] he gives details of ninety-four amputations performed by himself or his pupils, with only three deaths. (Two further deaths are excluded, viz., one from tetanus which was present before the operation, and the other in which the patient lost a great deal of blood, and where Burow did not himself complete the after-treatment.)

These cases were :—

Amputation of thigh (upper third)	9 cases (1 death).
,, ,, (lower third)	12 cases (2 deaths).
,, leg . . .	18 ,,
,, metatarsal and tarsal bones . .	7 ,,
,, close to shoulder .	4 ,,
,, upper arm (upper third) . .	5 ,,
,, upper arm (middle)	7 ,,
,, ,, (lower third)	7 ,,
,, fore-arm (upper third)	4 ,,
,, ,, (lower third)	24 ,,

[1] *Deutsche Klinik*, 1859.

[2] *Deutsche Klinik*, No. 24.

In 1859 Burow discusses the causes of death in cases where dressings are applied. He says that the bandages prevent the swelling of the soft parts which was so very noticeable in his cases. Plugs of blood clot of a certain size form in the veins. When the bandage is removed, the limb swells, and these plugs become too small for the veins, and may be carried away, and cause emboli and pyæmia. Burow also refers to the bad practice of using unclean sponges, instruments, &c.

In a later paper[1] (1876) he speaks of his great success in excision of the mamma. He had operated on 53 cases during the last ten years, without a single death. In these instances, however, he washed out the wounds immediately before stitching them up with a mixture containing chiefly acetate of alumina, which we now know to be a powerful antiseptic (see p. 139). He also dressed suppurating wounds with pledgets of lint soaked in this solution and covered with gutta percha; and he himself speaks of the powerful effect of the solution in arresting putrefaction. These cases were, therefore, not treated by the open method, but by the addition of an antiseptic substance to the discharge, in many cases, no doubt, by complete exclusion of bacteria, i.e., on the Listerian or aseptic principle.

The chief advocate of the open method in England has been Dr. HUMPHREY, of Cambridge, whose great success at the Addenbrooke's hospital is well known. In a lecture on the treatment of wounds without dressings (1867),[2] he says: 'What is the great requisite—the sine quâ non—for the immediate union of wounds? Obviously, adaptation of the cut surface. Unless these are placed in apposition, they cannot easily unite. Hence the want of union is commonly proportionate to the want of apposition of the surfaces; and one great object in the treatment must be to secure as exact an apposition of the cut surfaces as possible, and to reduce to the minimum all substances, blood, dressings, ligatures, &c., that may intervene between them, and keep them apart. In the case of small and superficial wounds, there is little difficulty about this. Some simple method answers, and they heal at once. In large wounds, the edges are brought together by means of sutures.

[1] *Archiv für Klin. Chir.* XX. [2] *British Medical Journal.*

and afterwards no application is employed with the exception of warm water dressing or poultices (rather opposed to the antiseptic element!), which are occasionally applied after a few days.

On the influence of air on wounds, he writes: 'But I am sometimes asked, is it not better to exclude the air? Is not the air a source of mischief? Is not the well-doing of subcutaneous wounds due in great measure to the exclusion of the air? To this I reply that it is certainly well to exclude air from the interior of the wound, where it keeps the surfaces apart, and promotes the decomposition of the fluids, and perhaps of the solids. On the surface of the wound, however, that is on the skin, it does not act as an irritant, and we should scarcely expect that it would. We have long been in the habit of leaving wounds of the face uncovered, and they usually heal remarkably well. Why should we not follow the same practice in other parts of the body?' Humphrey used acupressure instead of ligatures.

The open method was for a time pretty generally adopted in Germany and Russia, and to Krönlein's work on the subject I shall have to refer later. Of late, however, it has almost entirely given place to one or other mode of treatment with antiseptics.

Healing by Scabbing.

As we have already seen at p. 279, this is a nearer approach to aseptic treatment than the open method. As I have previously stated (p. 310), John Hunter made extensive use of this principle in the case of small wounds. He sometimes applied various powders to aid the formation of the crust.

Since Hunter's time numerous attempts have been made to carry out his principle of applying powders to aid the formation of crusts, but though they have answered very well in small wounds, they have not succeeded in large. Discharges collect beneath them, and tension is the result.

Among the substances employed in this way, have been various antiseptics, and of these may be mentioned Neudörfer's[1] powder. He uses salicylic acid, in the form of a powder,

[1] *Die Chirurgische Behandlung der Wunden*, 1877.

mixed with other substances, such as starch, zinc oxide, &c. He dusts the wound with the powder, and rubs it up with the fluids of the wound. Thus a mass is formed, which is again rubbed up with fresh powder till no more fluid comes; then over the whole some pure salicylic acid is applied, and a bandage. This crust is left on, and if on pressure on it some fluid exudes, more powder is applied. This is generally necessary five or six times during the progress of a case. This method is really only of use in small wounds. The objections to its use in large wounds are that the pain is great, the material is expensive, and there is very apt to be confinement of discharge. Neudörfer states that there is no bad smell in the cases treated in this way. Among them he has had no bad case of pyæmia, hospital gangrene or tetanus. He cannot say much about erysipelas. As a rule, in the cases which were attacked, it was generally some other part of the body which was affected.

Both JAMES MOORE and ASTLEY COOPER tried to obtain crust formation by means of *charpie*, and the same sort of method was employed by SYME. The latter surgeon, after bringing together the edges of the wound, covered it with a piece of dry lint, which was left on for several days.

The *combination of a piece of lint with an antiseptic* was the method employed by BENNION of Oswestry. To quote Wm. Adams, who mentions this treatment in his publication in 1857,[1] Bennion's method in compound fracture was—

'1st. Immediate reduction; and in securing a good apposition of the fractured surfaces, he would frequently employ more force than many surgeons might think prudent, so that he very rarely had occasion to saw off any portion of bone.

'2nd. He cleared away all the blood from the wound, considering that it interfered with the reparative process, and brought the edges of the wound into apposition. He then covered the wound with a large bit of lint, saturated with compound tincture of benzoin, and bandaged the entire limb, firstly by itself, whilst extension was being kept up by an assistant, and then to a well-fitting splint adapted to the case; in the lower extremity he used a straight splint.

'3rd. He put the patient at once under the influence of

[1] *Subcutaneous Surgery*, 1857.

opium, upon which he placed great reliance, and kept up its action for a considerable time, according to the circumstances of the case.

'4th. He never disturbed the first dressing or bandage, unless urgent symptoms indicated the necessity for so doing. If such symptoms did not appear, he would allow the first dressing to remain a month.' It seems that Bennion had some very remarkable successes by this method.

Mr. LISTER'S first attempts to form a crust by means of *carbolic acid* have been already alluded to on p. 128.

Crust formation has been aided by the *drying of the discharges.* This occurred in GUYOT'S cases, though he did not aim at it. It was, however, the principle in the often misconstrued method of BOUISSON,[1] the so-called 'ventilation of wounds.' Bouisson's method consisted in blowing air on to the surface of a wound by means of a caoutchouc bag. He generally directed the patient to do this himself. The 'ventilation' lasted a variable length of time, according to the extent of the surface and the quantity of fluid. A quarter of an hour generally sufficed for each *séance*, and this was repeated four or five times a day. The first crust which formed was rarely sufficiently thick or firm, but by-and-by the crust became of sufficient thickness, and when this was obtained, the ventilation was stopped. If the crust seemed to be confining the discharge and doing harm, it was softened and removed, and the process begun again.

Such was Bouisson's method of obtaining a crust, and as this method has been often quoted as shewing what a slight effect atmospheric dust exercises on a wound, it is interesting to note the advantages he claims for it. These advantages are: 1. A sedative action. Bouisson says that the pain diminishes so much that the patients often continue the ventilation for a long time. 2. Astringent action, so that the vessels are constricted and there is less tendency to inflammatory congestion. 3. Drying action, i.e. the formation of a crust. 4. This crust exercises a protective action, protecting the wound from the air, and also lessening the quantity of secretion. 5. Antiseptic action: from the evaporation and

[1] *Comptes-rendus*, t. 47, 1858.

concentration of the fluids, decomposition cannot occur. He says: 'Le contact de l'air et de ce liquide' (the discharges from a wound) 'sous l'influence de la chaleur animale détermine la formation des produits putrides sulfhydriques ou ammoniacaux, isolés ou combinés, et leur abondance contribue à faire naître des phénomènes typhiques ou ataxiques chez ceux qui se trouvent placés dans ces fâcheuses conditions. La ventilation des surfaces suppurantes a nécessairement pour effet de prévenir ou d'empêcher de pareils résultats, et l'on peut dire qu'à ce titre elle est autant un moyen d'hygiène générale dans les hôpitaux qu'un artifice local pour prévenir la resorption putride.'

In his conclusions, in which he lays stress on the points already mentioned, he says that wounds and ulcers so treated heal sooner and with fewer accidents, either primary or secondary, than wounds treated with dressings, or with any sort of medicament. He lays special stress on the avoidance of danger from a septic state of the pus. He recommends the use of this method in all small or medium sized wounds, ulcers, burns, &c.

The last mode of forming a crust which it will be necessary to mention is by *cauterisation*. This has been resorted to by various surgeons; first, I believe, by FAURE. An exhaustive paper on the subject was published in 1843 by BONNET.[1] The cauterisation may be done in various ways, either by caustics or by the actual cautery. Of caustics Bonnet finds that the Vienna paste or chloride of zinc alone are the best. The latter was extensively used by M. CANQUOIN and also by M. GENSOUL. Bonnet remarks that after its use he has never seen decomposition occur in the wound, or phlebitis and pyæmia. Bonnet preferred the actual cautery for various reasons. He attributed its good effects in the main to desiccation of the tissue, and he used the actual cautery more especially in wounds which had taken on a bad action.

Irrigation and the Water Bath.

These are very efficient antiseptic methods, and indeed, I believe, stand next in order of efficiency to strict aseptic mea-

[1] See also *Traité pratique de la Cautérisation, d'après l'enseignement clinique de A. Bonnet de Lyon.* Par R. Philipeaux, 1856.

sures, more especially when the water used is impregnated with an antiseptic.

Without entering into the older history, I may mention that OTIUS in 550 praises the use of cold water in recent injuries, and in some cases used more or less continuous irrigation. Again PALATIUS in the sixteenth century recommends the use of continuous irrigation with cold water, and speaks of having in this way obtained excellent results.

Two centuries later SMITH [1] in England (1725) and LAMORIER in France (1732) advocated its use. Lamorier, indeed, preferred prolonged water baths to irrigation. LOMBARD and PERCY used irrigation, and the former also employed water baths pretty extensively. It was not however till the publications of JOSSÉ and BÉRARD on irrigation in 1835, and of MAYOR on the water bath about 1836, that these methods were fully recognised.

During the further progress of this method, the various questions which have been agitated are, as we shall see, whether the irrigation or the water bath was the best; whether these ought to be continuous or intermittent; what the temperature of the water should be; and whether or not some antiseptic substance should be added to the water.

The first, as I have said, who in recent time called attention to this subject, was JOSSÉ; his work containing his views was edited by his son in 1835.[2] His reason for using irrigation was, that in this way a continuous and more perfect application of *cold* could be obtained. He used it in all the larger wounds, and in all inflammatory states. The apparatus which he employed was essentially the same as that now in use.

In the same year BERARD [3] (junior) published, quite independently, a paper on the use of cold water as an antiphlogistic. He also employed irrigation in order to obtain a constant diminution of temperature, which cannot be done, he thinks, by the application of cloths dipped in cold water, even though they are impregnated with substances which volatilise quickly. He

[1] *The Curiosities of Common Water*. 1723.

[2] *Mélanges de Chirurgie pratique, &c.*, 1835.

[3] *Mémoire sur l'emploi de l'eau froide comme antiphlogistique dans le traitement des maladies chirurgicales*, 1835.

mentions several cases which did very well. The only complication, which he thinks is really to be dreaded, is death of the tissues from the continuous cold; but he says that this rarely occurs except in severe contusions.

In 1838 NIVET[1] published a paper giving some of the results of this method. He mentions, among other cases, nine compound fractures so treated, of which seven recovered, five of these without any local or general inflammatory symptoms. He mentions various accidents which may occur, of which the following are examples: Pain (this generally soon disappears); paralysis (observed in one case); œdema (combat by bandaging); phlebitis (very rare); inflammation of the parts around the fracture; gangrene (generally only where the parts are much injured); anchylosis or stiffness (where the part is kept for a long time in one position).

About 1834 M. CHARLES MAYOR published his work entitled 'De la Localisation des Bains sur les diverses parties du Corps humain.' I have been unable to obtain this work, and have had to content myself with a work published in 1846 called 'Les Bains sans Baignoires et ramenés à leur belle simplicité.' His wish was to provide a permanent warm bath in imitation of nature, for he says: 'Is it with pledgets of *charpie*, with rags spread with various ointments, with compresses and with bandages, cold and dry, that nature carries out the cure? Nature, wise and simple nature, contents itself with calling to its aid a warm and permanent bath, which furnishes it plentifully with lubricating fluids, thoroughly diffused over the wound, and constantly being renewed.' He points out that this bath keeps up an equable temperature, and that nature uses it after removal of the tonsils, after operations for harelip, after tenotomy, cataract, abdominal section, &c., and it is to supply its place in external wounds that he proposes the use of the permanent warm bath. Mayor described a variety of apparatus for use in different situations. He did not place the whole body in the bath, but merely the diseased or wounded part. His apparatus was so constructed as to permit the renewal of the water without removal of the apparatus. The temperature of the water was about 22°–25° C. The baths

[1] *Gazette médicale de Paris*, 1838.

were employed for several days, till in fact there was no fear of the occurrence of accidents.

The advantages of irrigation were admitted by NÉLATON, and more especially by MALGAIGNE,[1] who thought it best to use it intermittingly, and so introduced 'intermittent irrigation.' Malgaigne considered that the temperature of the water ought to be regulated by the first sensations of the patient, and that as a rule it ought to be tepid, i.e. about 20° C. Malgaigne's term 'intermittent irrigation' applied, however, more to a sort of water dressing than to irrigation proper, the cloths applied to the wound being soaked in water and changed at frequent intervals.

Although at first the use of water was greatly resorted to, it soon began to go out of fashion, chiefly, it was said, on account of the accidents to which it might give rise, and when AMUSSAT[2] *fils* wrote his elaborate thesis on this subject in 1850, it was but little practised. In this thesis Amussat deals chiefly with the question of the temperature of the water. He and his father, after long investigation of the subject, came to the conclusion that many of the bad effects attributed to the employment of water were due to its being used cold. They found that cold water from 0°–10° C., was a powerful antiphlogistic and sedative, but that it was liable to do harm. Water at 30°–35° C. relieves pain in an inflamed part and promotes suppuration: it however tends to cause congestion. Water at 18°–25° C. has all the advantages of cold water without its disadvantages: it abstracts heat, but does not cause congestion. They recommend the use of soft water and water containing few salts. Amussat considers that immersion is the most effectual mode of applying water; next to that comes irrigation, and, lastly, a form of water dressing which he describes in detail.

Looking on irrigation and the water bath simply as a means of abstracting heat, ESMARCH,[3] in 1860, writes very strongly in favour of cold, and he recommends iced water and even ice itself. He mentions several cases in support of his views.

[1] *De l'irrigation dans les maladies chirurgicales.* Thèse de concours, Paris, 1842.

[2] *De l'emploi de l'eau en chirurgie*, 1850.

[3] *Archiv für klin. Chirurgie*, 1860.

In 1872 ISAMBERT [1] also writes in favour of cold irrigation, but as there is nothing new in his work, I need not refer further to it.

While this discussion as to the temperature of the water was being carried on, others were using the water bath with the view of excluding the air and avoiding putrefaction. I refer especially to the attempts of STROMEYER, LANGENBECK and VALETTE.

In spite of the writings of Mayor and Amussat, the permanent water bath made but little progress till it was taken up by LANGENBECK. Since 1839 Langenbeck had made several attempts to exclude air from wounds. These attempts resembled those of Jules Guérin and others, previously mentioned, but they were not successful in Langenbeck's hands, owing to the confinement of the discharges. Accordingly, during the Schleswig-Holstein war in 1849, Langenbeck commenced the use of the warm water baths with the view of excluding the air. At the same time also STROMEYER made similar experiments. During the next few years Langenbeck perfected his method, and at length, in September, 1855, he published an elaborate paper on the subject.[2]

Just before the publication of Langenbeck's paper, PICARD,[3] who had been visiting Langenbeck's *clinique*, published an account of his practice and results in the 'Gazette hebdomadaire de Médecine et de Chirurgie.' In this paper he speaks very highly of the method, showing how it relieves pain, how there is but little local inflammation or constitutional disturbance, how the appetite remains good, how granulations form rapidly, so as indeed at times to be exuberant, how there is no odour, and how this method seems to protect the patient against the chance of pyæmia better than any other. In contrast with this, he describes, in a very graphic manner, the course of a wound treated by the ordinary methods—a course common at that time, and indeed, as I have myself seen, by no means uncommon at the present. He says: 'Déjà, grâce au chloroforme, la

[1] *Considérations cliniques sur les bons effets de l'irrigation continue et des applications froides renouvelées dans le traitement des fractures avc plaie.* Thèse, 1872.

[2] *Deutsche Klinik*, No. 37, 1855.

[3] *Gazette hebdomadaire de Médecine et de Chirurgie*, 1855.

douleur disparaît au moment décisif; l'homme vivant est insensible sous le bistouri; ses lamentations ne viennent plus interrompre l'opérateur. Mais après avoir dépassé le seuil de la salle d'opération, dès que la torpeur anesthésique s'est dissipée, le patient, abattu, affaibli, voit s'ouvrir devant lui tout un avenir de souffrances. La douleur, un moment comprimée, se déchaîne avec violence. Plus de repos, plus de sommeil. Les terribles secousses de la fièvre inflammatoire épuisent le malade et compromettent le succès de l'opération. Les tortures quotidiennes du pansement viennent aggraver cet état général. Qui n'a pas eu le cœur serré en entendant les plaintes arrachées par le moindre mouvement de la partie opérée, par le tiraillement des brins de charpie agglutinés, par la pression grossière et inégale du bandage, sans compter l'atmosphère infecte dans laquelle le malade et ses voisins sont condamnés à vivre, les souillures des draps et des couvertures? Enfin, la nature est dérangée dans son travail réparateur; chaque tour de bande change les conditions de la plaie, et le chirurgien ne peut lui-même diriger la guérison '

Langenbeck placed the wounds in the bath immediately after operation. Where possible, he stitched up the wound, leaving a space at the angle through which discharges passed, and through which the ligatures were brought out. During the first twenty-four hours, the limb was simply suspended in a bath, and was not arranged in one of the apparatuses which are apt to constrict the part and cause bleeding. When the edges could not be brought together, *charpie* and a bandage were applied for the first twenty-four hours to prevent bleeding.

The permanent bath was continued till granulation was complete, and till epidermic formation had commenced. The limb and granulations were apt to become œdematous, but this subsided in a few hours after the removal of the part from the bath. The apparatus was emptied night and morning, and the walls of the vessel carefully sponged with water containing chlorinated soda or chloride of lime. When the stump had been closed at first, the wound was daily injected along the course of the ligatures. The temperature of the water was at first 10°–12° C., but it was raised, as the patient could bear it, to 31°–37° C. The skin of the stump had a thick

layer of grease over it, to prevent the action of the water on the epidermis. Where the whole body had to be immersed, the immersion was continued for a half to one hour daily in water at the temperature of 34° C. Water dressing was employed towards the end of the case.

Langenbeck gives the following as the advantages of the permanent water bath :—

1. It diminishes pain in the wound, and also does away with all painful dressings.

2. The intensity of the fever is less.

3. Stagnation and decomposition of discharges are completely prevented.

He says that in wounds so treated, union by first intention is rare, though not unknown.

Among the cases treated in this way he had had, during a period of five or six years, no instance of pyæmia, although, as he says, some of the cases were very well suited for its occurrence, and although pyæmia was prevalent at the same time in other wards and even in cases in the same ward. He says: 'Cooling of the wound, retention and putrefaction of the discharge, and miasmatic influences (hospital air) are the causes of pyæmia. These noxious agents can be completely excluded by the careful employment of a water bath.' He also believes in the spontaneous occurrence of pyæmia, through constitutional and blood defects, and he does not think that these rare cases would be prevented. Then, also, decomposition of the discharges in a complicated wound is quite possible, even in a water bath, and might lead to pyæmia. He mentions several (7) cases to show the good results. It is interesting to note that one of these—the only fatal case—was one in which, during the removal of a tumour, the knee-joint was opened. Violent inflammation and suppuration of the joint followed and the patient died on the twenty-first day.

Langenbeck's paper was followed a month later by one by Dr. Fock.[1] He confirmed Langenbeck's good results, comparing together cases of Pirogoff's amputations treated by the warm bath, with others treated by the ordinary methods, much to the advantage of the former. He mentions also that Dr.

[1] *Deutsche Klinik*, Oct. 1855.

WAGNER of Dantzic, who had adopted it, reported that formerly pyæmia was very prevalent in that hospital, but that since the introduction of the warm water bath it had almost entirely disappeared.

In a paper in 1856, Picard[1] mentions that GOSSELIN and LAUGIER had adopted this method, and Fock also refers to ULRICH.

In October 1856, ZEIS of Dresden published a note entitled 'Beitrag zur Würdigung des permanenten warmen Wasserbades,'[2] in which he advocates the use of a still higher temperature than that recommended by Langenbeck. Zeis finds that a permanent bath below the body temperature is seldom endured by the patient. The best temperature is 37°–42° C. For amputations 35°–37° C. may be sufficient.

It is unnecessary for me to pursue the history of Langenbeck's method further. It is the one generally adopted where immersion is employed, and for some time it was very extensively used in Germany. It has of late gone out of use, not because its results are not good, but because it is difficult and inconvenient of application, and because it can now be replaced by more effectual and simpler methods.

Before leaving this subject, however, I must do justice to a method in which the antiseptic element was more fully recognised, and in some respects better carried out, but which has, very strangely, not attained any prominence, probably because the name of its author was eclipsed by the better known name of Langenbeck. I refer to the method proposed, and, while he had opportunity, carried out by VALETTE of Lyons.[3]

Valette's view was the following: 'The safety of subcutaneous wounds sufficiently demonstrates that the presence of the air in the wounds is the primary cause of their bad course.' 'How does the air act? This is a delicate question, but the following is the explanation which appears to me to be the fairest. As soon as the operation is finished the blood and the liquids which collect on the surface of the wound, decompose under the influence of the contact of the air. Sulphuretted hydrogen

[1] *Gazette hebdomadaire*, 1856. [2] *Deutsche Klinik*, October, 1856.

[3] *Sur une nouvelle méthode de pansement des grandes plaies* (*méthode anaéroplastique*), *Gazette hebdomadaire*, 1856.

and putrid gases, if you will pardon this expression, are formed and absorbed in part, and exercise a deleterious influence on the body. In a word, there is a true poisoning of the patient, which the constitution often withstands, though not always, especially when the patient is placed in unfavourable hygienic conditions, as is the case in the wards of our hospitals. In these cases the phenomena of cicatrisation do not proceed in a favourable manner, suppurative phlebitis occurs, and pus passes into the circulation; purulent absorption is an accomplished fact, and death is, one may say, an almost inevitable consequence.'

To avoid these dangers Valette immersed the wounded part in baths containing water impregnated with tincture of benzine, creosote, alcohol, or perchloride of iron. The last gave the best results. There was no putrefaction of this fluid, and he seems to have had great success. He was especially struck with the absence of traumatic fever.

PUPIER,[1] who wrote his thesis in 1855 on Valette's method, mentions a number of cases which did well, and states that, in order to prevent the water from getting too warm, it was in one case changed as often as thrice daily.

Valette had only a limited opportunity for carrying out his experiments, for after a short time he was placed in charge of a children's ward, where he could no longer practise this method.

[1] *D'un traitement consécutif spécial des amputations.* Thèse, 1855.

CHAPTER XVI.

HISTORY OF ANTISEPTIC SURGERY—(*concluded*).

Use of antiseptics: Alcohol—older writers—Nélaton—Hutchinson: Glycerine—Demarquay: Chlorine: Chloride of zinc: Iodine: Iodoform: Chlorate of Potash: Perchloride of iron, &c.: Coal tar—Corne and Demeaux—Report of the commission: Coal tar saponiné—Lemaire—his views on putrefaction: Carbolic acid—Lemaire—Lemaire's position in regard to aseptic surgery—Lister: Further phases in the history of this subject: Objections to Mr. Lister's claim as originator of the aseptic method—Simpson—Neudörfer. Modifications of the method. Substitutes for carbolic acid. Objections to the aseptic method.

The last point to which we have to refer is the use of *antiseptics*. As we have already seen, various antiseptics have been in use for many centuries as applications to wounds, and some surgeons indeed have ascribed the good results, derived from their use, in the main to their anti-putrescent properties. In spite, however, of the attempts of surgeons at different times to draw attention to these properties, it was not till the publication of Corne and Demeaux's method in 1859 that general interest in the subject was aroused.

Among the substances formerly employed, *alcohol* was probably the one in most extensive use, and after the subject of antiseptics came to the front, it was freely employed in some quarters. As I have just said, alcohol was formerly used in great quantities, but then it was generally combined with other substances, though it probably was really the active basis of the compounds so prepared. Thus the balsam of Fioraventi—a celebrated application to wounds—consisted of turpentine, myrrh, aloes, a large quantity of alcohol, and several unimportant substances. Many of the various lotions which have been most popular at different times had a similar constitution.

BATAILLÉ[1] published a little pamphlet in 1859 advocating the use of alcohol, but it was most extensively adopted by NÉLATON in 1863, and also by LE FORT.

CHEDEVERGNE[2] in 1864 published a detailed account of Nélaton's practice with the results which he had obtained. Nélaton's method was to soak *charpie* with camphorated alcohol, and apply it over the wounds in thick layers. These were either kept wet by frequent arrosion, or were covered with an overlapping layer of impermeable tissue, or in some cases the dressing was changed twice daily. Patients treated in this way remain in good health, the edges of the wounds adhere, no granulations are formed, but the wound becomes covered with a layer of lymph under which cicatrisation rapidly occurs. This is the rule, though not an absolutely constant one. In open wounds the granulations can scarcely be seen. There is an abundance of coagulable lymph, almost no appreciable suppuration, and no bad smell. In some cases the *charpie* becomes embedded in the coagulated albumen, and the whole dries up and forms a crust.

Chedevergne says that, for more than a year, operation wounds, and injuries, had been treated with camphorated alcohol, or with pure spirits of wine, and that during that time pyæmia and other infective diseases had been almost entirely absent. Of forty-eight patients who had undergone severe operations, only three died; one of tuberculosis, one of hypostatic pneumonia, and one of pyæmia.

ROCHARD[3] gives later statistics, in which he shows that there were only two cases of pyæmia, and five of erysipelas. Good success in the Hôpital des Cliniques!

On a previous page (p. 269) I have referred to Mr. HUTCHINSON'S method of using alcohol. He mixes it with acetate of lead somewhat after the manner of Astley Cooper.

Glycerine was used pretty extensively in England after 1840, but was introduced in the treatment of wounds mainly by DEMARQUAY in 1855.[4] He first employed it in cases of hos-

[1] *De l'alcool et des composés alcooliques en Chirurgie.* Par MM. Bataillé et Juillet, 1859.

[2] *Bulletin général de Thérapeutique*, vol. lxvii. 1864.

[3] *Loc. cit.*

[4] *Gazette des Hôpitaux*, October, 1855.

pital gangrene, and he found that they rapidly began to improve under its use. He then extended the treatment to ordinary wounds. He claimed all sorts of advantages for it. In 1859, more especially, he stated that it was a specific against erysipelas, pyæmia, hospital gangrene, &c. He thought that it was the best antiseptic substance, and indeed a universal panacea. Since that time it has, however, been extensively tried and found wanting, and has, as Rochard says, for a long time been justly relegated to the modest place in therapeutics which nature had assigned to it.

Chlorine and its compounds have been used as disinfectants ever since its discovery in 1774. Guyton, in 1795, recommended its use in hospitals by fumigation, in order to destroy miasms. The Liquor de Labaraque, which attained considerable notoriety as a disinfectant, consisted mainly of chlorinated soda. In more recent times the chlorine compounds, more especially chlorinated soda and chloride of lime, have been employed in the treatment of wounds, the wounds being washed or syringed out with the solution, and dressings applied which were frequently moistened with the same. Hervieux applied a sponge soaked in the solution to the wound, and this, according to him, not only disinfected the discharge, but also absorbed it. M. Guersant also used it largely.

Chalvet [1] recommends the introduction of chloride of lime into the superficial layer of the *charpie*. Chlorine is given off from this, and prevents the decomposition of the discharges.

We have already mentioned *chloride of zinc*, which, as we have seen, was extensively employed as a caustic by Canquoin and Gensoul. Bonnet also used it, and remarked on the absence of putridity in the wounds made by it. In more recent years Campbell de Morgan has again used it as a caustic, and also made the same remark as to its antiputrescent powers. It thus came to be employed as a lotion in the treatment of wounds, but it has never become extensively used. The chief use to which it is put nowadays is by Mr. Lister in disinfecting foul ulcers or sinuses (see pp. 65 and 116).

[1] *Des désinfectants et de leur application à la Thérapeutique et à l'Hygiène. Mémoire de l'Académie impériale de Médecine*, vol. xxvi. 1863.

Velpeau in 1859 stated that *Iodine* had been in use as a topical application for over thirty years. On the contrary, Duroy asserted that it was only as a consequence of a communication made by him in 1854 that iodine was first employed in wounds. Iodine has at times been used in the practice of various surgeons. Thus Mr. Spence of Edinburgh at one time painted the surface of stumps with iodine, and thought that he got good results from it; however, he soon took to painting stumps with other things, and the iodine fell into disuse.

The only preparation of iodine which is now employed in the treatment of wounds is *Iodoform*, which is of great use in chancres or foul ulcers. I have already referred to the method in which it is applied in the latter cases (pp. 65 and 94).

Various metallic salts have been from time to time introduced, such as *Chlorate of Potash* by Milton. This acts very well in ulcers in the mouth, but is not of any very great value.

Perchloride of iron was employed as a disinfectant by Deleau, and I have already referred to the use which Valette made of it (p. 346).

Nitrate of lead, *subnitrate of bismuth* and other powders have also been spoken of at various times.

During the middle of the present century there had been constant efforts made in various directions to diminish the great mortality in French, more especially in Parisian, hospitals by means of some method of wound treatment. We have seen how at first it was chiefly the action of the air on the wound which was guarded against, but in 1859 it had come to be fully recognised that the chief thing which was required was to prevent decomposition of the discharges. It is therefore not a matter of surprise that when Corne and Demeaux in that year brought forward a powder which they believed to be capable of attaining this object, attention was at once directed to the subject, and not only did debates occur in the scientific societies, but experiments were also made in the various hospitals with this and other antiseptic substances.

Already in 1815 the antiseptic and disinfectant properties of *coal tar* had been recognised by Chaumette, and Guibourt (1833) and Siret (1837) also wrote on the subject. In 1846

BAYARD introduced an excellent disinfectant, of which the chief ingredient was coal tar.

In 1858, M. CORNE took out a patent for a powder which he had for some time employed as a disinfectant, and in the same year DEMEAUX[1] used this powder in the treatment of wounds. The powder employed by Corne and Demeaux[2] had the following composition: of ordinary plaster of commerce in very fine powder, 100 parts; of coal tar (obtained from distillation of coal in the manufacture of gas), 1 to 3 parts. These substances were readily mixed together in a mortar, and then olive oil was added till they acquired the consistence of paste. This paste was applied to wounds, more especially to foul wounds, and it was found that it disinfected (deodorised?) the discharges at once, and that it also absorbed them. They stated that, 'the action of this disinfectant substance seems to arrest the work of decomposition; it keeps away the flies, and prevents with certainty the production of worms.' Their experiments were made partly in private practice, and partly in Velpeau's wards. VELPEAU observed that in the cases treated in this way suppuration was also diminished.

In the discussion which ensued at the following meetings of the Academy, various opinions were expressed as to the use of antiseptics in general, and as to the action of this particular powder.[3] M. CHEVREUL found that the coal tar powder did not destroy the odour, but rather cloaked it, while, on the other hand, hypochlorite of lime in large quantities made it disappear. He thought however that the coal tar powder would act in preventing putrefaction. M. BUSSY called attention to the fact that other disinfectants were in constant use, and were also employed to prevent putrefaction. Such were carbon, chloride of lime, soda or potash, creosote, &c. M. RENAULT stated that as long ago as 1840 he had published a memoir on disinfection of wounds; for at that time he had come to the conclusion that the putrefaction of blood and discharges, and the contact of these putrid materials with the wound, were the cause of the bad after-consequences. He had found that hypochlorite of lime was the best, though in some cases the

[1] *Union médicale*, 1860. [2] *Bulletin de l'Académie de Médecine*, 1859.
[3] *Ibid.*: see also *Gazette médicale de Paris*, 1860.

disengaged chlorine irritated the lungs. Hence he welcomed the coal tar powder, but he stated that vegetable tar acted as well and had a less disagreeable odour. CALVERT had previously shown that carbolic acid, which was present in coal tar, was a powerful disinfectant, and, at a later meeting of the Academy, he stated that it had been used in Manchester in 1857, in the preservation of dead bodies, with success. DUMAS had stated, at a previous meeting, that carbolic acid was present in coal tar in small quantities and that the least trace of carbolate of soda was sufficient to preserve animal matters.

After the reading of Corne and Demeaux' paper and the subsequent discussion in the Academy, a commission was appointed by the Academy to enquire into the matter. This commission consisted of CHEVREUL, J. CLOQUET and VELPEAU. As the result of their investigations, they concluded that this powder was most useful for disinfecting substances, but that it was not a convenient application to wounds, that its odour was disagreeable, that it had often to be renewed and that it soiled the linen. LEMAIRE also adds that it solidifies and prevents the escape of pus.

The commission tried various other substances which had been mentioned, such as glycerine, sugar, chlorate of potash, &c., but found that they were not much better than ordinary cerates. Tincture of iodine was found to act fairly well, but to cause too much pain ; chlorine and the hypochlorites were good. Sub-nitrate of bismuth or perchloride of iron used in the form of an ointment (8 grains of the liquor to 30 grains of lard), were excellent applications.

Corne and Demeaux' powder soon disappeared from the list of remedies, but nevertheless attention was attracted by it to the subject of antiseptic applications to wounds.

Among those who spoke at the debates was LE BŒUF, who in 1850 had found that substances insoluble in water, but soluble in alcohol, could be made into an emulsion by the addition of saponine. Such emulsions were very fine and very stable. Le Bœuf proposed that coal tar should be emulsionised by the aid of this tincture of saponine.

JULES LEMAIRE at once took advantage of Le Bœuf's suggestion, and in 1860 he published a paper on 'Coal tar sapo-

niné.'[1] The tincture of saponine, to which reference has just been made, is an alcoholic extract of the bark of *Quillaya saponaria* and contains other substances besides saponine. A tincture of 'coal tar saponiné' was made by mixing together 1 part of coal tar with from 2 to 4 parts of tincture of saponine. With this an emulsion was made by mixing 1 part of the tincture with 4 parts of water. This emulsion retained all the properties of coal tar, and is the substance which was used by Lemaire. Lemaire investigated the activity of its various constituents, and found that it contained *saponine* which acts as an antiseptic; *alcohol*, also an antiseptic; *carbolic acid*, a powerful antiseptic, but apt to cauterise the tissues; *benzine*, which is an irritant; *naphthaline*, which is a sedative, and, in Lemaire's opinion, 'modifies and tempers the action of the other substances.'

Lemaire applied this emulsion very extensively in the treatment of a variety of diseases, and also to putrid wounds and ulcers. The results of its use in the latter cases were— 1. Disinfection of the wound; 2. The wound assumes a rosy aspect; 3. The sloughs become detached with greater facility than usual; 4. It only exceptionally causes pain. Lemaire also states that it acts powerfully in reducing the quantity of pus secreted. This emulsion was applied in the same manner as other lotions. The wound was washed with it, and then dressed with charpie soaked in the emulsion. I cannot find any case illustrating its use from the time of operation. There is no sort of system described, and Lemaire seems to have used it merely as a disinfectant and as a good application to wounds. In fact he treated the wound with antiseptics, but not aseptically. Numerous letters from various surgeons are published in Lemaire's book shewing that smell was destroyed at the time of the application, but that in some cases the discharge became foul before a fresh application was made.

His views on the germ theory of putrefaction are of the greatest interest, as he was undoubtedly the first who, recognising that theory, applied it to practice. He says, 'the wound which suppurates, as I hope to demonstrate presently, is a secreting surface, the products of which become altered under

[1] *De coal tar saponiné*, 1860.

the influence of air and give birth to a series of phenomena due to one and the same cause, fermentation.' He believed that pus at the commencement is simply serum of the blood containing fibrin, that fermentation occurs in this from contact with the air and that pus corpuscles correspond in their nature to yeast cells and are the result of this fermentation. Now coal tar saponiné at once arrests this fermentation and consequently this formation of pus cells. I have already referred to Lemaire's experiments on putrefaction and other fermentations (p. 218). He concludes that the two principal properties of coal tar saponiné are disinfection and arrest of fermentations. 'A third important property which seems to me to be the cause of the arrest of the fermentations, is the toxic action which it exercises on vegetables and on the lower animals.' It favours healing by disinfecting the wounds and arresting the fermentation of pus. He concludes that the germ theory of fermentations is true, and that coal tar arrests and prevents fermentation by its toxic effect on the lower organisms which cause them. He says again: 'These facts make me think, that it is on account of its toxic properties on vegetables and on the lower animals that coal tar can prevent or arrest fermentations. The opinion of Schwann and of several other physiologists, a view which in the present day has received great support from M. Pasteur, appears to me to be true.' . . . 'As the ferments appear to be the infusoria and the microscopic vegetable organisms which exist in abundance in the atmosphere, and as the "coal tar saponiné" destroys them, let not physicians forget this precious property. *Perhaps it may permit them to make important discoveries, and to render a great service to humanity.*'[1]

While Lemaire was using this coal tar saponiné he was also experimenting with carbolic acid. He found that carbolic acid could form a 5 per cent. solution in water, and could also be dissolved in oil and alcohol. He found too that it was much more powerful than coal tar, but that its volatility and its powerful action on wounds were disadvantages. On the other hand, with carbolic acid he had a solution of known and constant strength, which mixed with the discharge and soaked into the

[1] The italics are mine.

tissues more readily than the emulsion of coal tar. He published a work on the subject in 1863, called 'De l'acide phénique,' and such was the interest excited in France by this work that in 1865 a second edition appeared.

The greater part of his work is taken up in discussing the germ theory, which he advocates, but the experiments, though numerous and laborious, are so crude and imperfect that I have not been able to make any use of them, though I had much wished to do so, if only for their historical interest.

In applying carbolic acid to medicine and surgery, he seems to have been guided by no principle or rule, but simply to have applied it empirically. Thus the following is a list of some of the diseases in which he used it: acne, anthrax, asthma, cholera, dysentery, ecthyma, eczema, erysipelas, intermittent fever, typhoid fever, herpes, impetigo, lichen, purulent ophthalmia, pityriasis, phthisis, prurigo, sycosis, cancerous ulcers, tænia, smallpox, &c. In cases where it did good it was supposed to have killed germs. He used it also in ulcers in the same way as the emulsion of coal tar, and where tortuous canals existed, the lotion of the strength of ·2 to 1 p. c. was injected. He says very little about recent wounds; indeed the following is about the only passage I can find. 'Pour mettre les solutions de continuité des tissus à l'abri de la fermentation il suffit de les couvrir dès le début avec des compresses, constamment imbibées d'eau phéniquée. Deux millièmes de l'acide phénique suffisait dans ce liquide pour obtenir ce résultat.' How very imperfect such a method would be, experience and experiment have since amply shown. He only mentions having treated one case of compound fracture, a fracture of the fingers, in which he used the emulsion of coal tar.

Here there is nothing which can be called a method. Lemaire realized what the causes of putrefaction were, but he made no attempt to exclude them, nor indeed did he make any systematic attempts to eradicate them after they had entered.

In 1865 DÉCLAT published a work on carbolic acid in which he claimed priority over Lemaire. This work contains no observations of any value, and his claim of priority is absolutely

without foundation, for Lemaire began his work before Déclat, and Déclat was well acquainted with Lemaire's work before publishing.

We must in a few words enquire what is Lemaire's exact position in reference to antiseptic surgery? how much does the present advance of antiseptic surgery owe to him? It is a question which for a time has been much debated and variously answered.

In the first place we find, as I have already stated, that Lemaire recognised the true basis of antiseptic surgery, the germ theory of fermentation, and also that he was the first to use carbolic acid extensively in the treatment of wounds. But we do not find any method of how best to add the antiseptics to the discharge or how best to exclude organisms altogether. All that we find is that certain results—absence of smell—were attained, and the *explanation* was that the carbolic acid had destroyed the causes of fermentation. Then, also, look at the aimless way in which he applied carbolic acid to all sorts of diseases. There was no definite plan, no clear purpose, in these attempts.

On the other hand, we must remember Lemaire's opportunities. He had no hospital appointment as surgeon; no opportunity, therefore, for seeing a sufficient amount of surgical practice to realise the necessity for systematic action. Would he have elaborated any aseptic, or even thorough antiseptic method had such opportunities been given him? I cannot say, but I doubt it, for his experiments shew the same want of purpose and imperfection in inference, while his views on pus cells, and many other points, show an imperfect appreciation of the elementary facts of physiology and pathology.

I am not, however, concerned with what he *might have been*, but with what he *was*; for indeed, as Carlyle well remarks, 'it comes that these same *would-have-beens* are mostly a vanity, and the world's history could never in the least be what it would, or might, or should, by any manner of potentiality, but simply and altogether what it *is*.' Lemaire *was* the first to use carbolic acid, and *was* the first to realise the truth of the germ theory as applied to wounds. *He was an advanced treater of wounds with antiseptics, nothing more.*

The use of carbolic acid in the treatment of wounds was for some time tried extensively on the continent, and in England it was also employed by one or two surgeons (Wood, Spence); but, applied in the way recommended by Lemaire, it failed to give satisfactory results and soon began to drop out of use. Indeed, as early as 1867, Dr. Hingston, who had been travelling on the continent, stated, at the Dublin Meeting of the British Medical Association, that 'he had found that the use of carbolic acid in surgery was now being discontinued in places where it was formerly in vogue.'

The result of these centuries of work on the subject of wound treatment may be summed up as follows. The old views, that wounds could not heal without active intervention on the part of the surgeon, had been thrown aside, and, after many oscillations of surgical opinion, it had at last become generally recognised that nature was the sole agent in the reparation of wounds; it had, however, become apparent to most men that the decomposition of the discharges in wounds was a source of danger to the patient so great as to demand active interference for the purpose of preventing it as far as possible. The various attempts which were made with this aim are most interesting and important, although they were all more or less ineffectual. On the supposition that the gases of the air were the active agents, numerous methods (occlusion, crust formation, substitution of other gases, &c.) had been devised for the purpose of excluding the air, but these had all failed in producing any permanent benefit; it was rendered clear from these researches that the gases of the air were not the injurious elements. Subcutaneous surgery, though based on this erroneous view, had become firmly established, and it was universally recognised that, if an operation could be done subcutaneously, it was to a great extent free from danger; unfortunately, the applicability of this method was very limited. Then, at first with the view of regulating the temperature of the wound, various forms of irrigation and water-baths were introduced. Some of the surgeons, however, who employed them observed that decomposition was less, and that this was due in great measure to the washing away of the discharges. Drainage had also to a certain

extent, though imperfectly, been introduced.[1] On the other hand, it had been found that, by the addition of various substances, hence termed 'antiseptics,' to the discharges of wounds, decomposition could be markedly interfered with, and, at the time to which we refer, in France more especially this idea was the most prominent in the treatment of wounds. One surgeon, indeed, Lemaire, had announced the view that antiseptics acted by destroying 'the vegetables and lower animals' found in these discharges, and which appeared from recent researches to be the active agents in producing decomposition. Lemaire, however, failed to follow out his views, and thus missed the discovery which was very shortly afterwards announced by Mr. Lister.

Such was the state of matters when Mr. LISTER, who had been working for more than two years independently of any knowledge of Lemaire's experiments, published his first papers on aseptic surgery, which at once threw a flood of light on the confused subject of the treatment of wounds. The development of his system in his own hands has already been traced. Mr. Lister's writings have stimulated surgeons to the study of the whole subject, and have led to the introduction of improvements in every detail of wound treatment. These improvements, acting on better principles, have brought even the older methods to a high state of perfection, and numerous researches have since been made which have enabled us to reduce to order and show the true principles underlying the various attempts at wound treatment which had been previously carried out.

It would be hopeless to attempt to follow out all that has been said and written on this subject since that time, and it would not only be hopeless but also profitless, for the greater part of the statements rest on imperfect understanding or knowledge of Mr. Lister's writings, and imperfect acquaintance with the scientific advances of the present day. In Germany, where science is more advanced than in other countries at the present time, this method has been almost universally accepted and introduced, and some of the leading German surgeons are amongst the most enthusiastic supporters of it. In our own

[1] Chassaignac: *Traité pratique de la Suppuration et du Drainage Chirurgicale.* 1859.

country on the other hand, the necessity for a protection against infective disease has not been so much felt, on account of the much better hygienic conditions of our hospitals; while, owing to our so called practical nature, the scientific problems involved have been allowed to fall out of notice and have been imperfectly appreciated when noticed. Perhaps the greatest obstacle to the acceptance of the principles of antiseptic surgery is the confusion which has somehow or other arisen between the germ theory of fermentation and that of infective disease. Indeed at the present time nine surgeons out of ten, if questioned, would give the germ theory of infective disease as the foundation of the principles of antiseptic surgery, while, as we have seen, it in reality has no necessary connection with these principles at all. The common argument brought against the aseptic theory is, 'How is it that after such an operation as removal of a finger, or excision of the mamma, not performed aseptically, the patient does not as often die of infective disease as after amputation of the thigh?' Now such a question shows the confusion between the two germ theories. The real question to put is 'How is it that putrefaction does not occur in all wounds not treated with aseptic precautions?' This I have already explained in considering the principles of antiseptic surgery, but I have nothing to do, in looking at these principles, with the former question. In considering the results, I shall demonstrate that the exclusion of bacteria from wounds is followed by the avoidance of infective disease, but I do not care whether any one believes that bacteria are or are not the cause of the infective disease. All I would ask is that bacteria be excluded, because they cause fermentations; and if this be done, as I shall presently show, the causes of infective disease will also be excluded. If one prefers to go out of one's way to suppose that not the bacteria, but something else, excluded at the same time, are the causes of infective disease, well and good; it does not matter for the *principles* of antiseptic surgery.

There are, however, one or two phases in the history of this subject to which I must briefly allude.

Firstly, attempts have been made to show that Mr. Lister has no claim as introducer of aseptic surgery. Such statements

rest on misappreciation of the meaning of that term. Most of these writers look on aseptic surgery, and the treatment of wounds by carbolic acid as one and the same thing, and then they point out that Lemaire and others used carbolic acid before Lister.

The first attempt of this kind was made by Sir JAMES SIMPSON, in the 'Lancet' for 1867. He says: 'Mr. Lister in his letter to the 'Lancet,' October 5, states that all his recent visitors to the Glasgow Royal Infirmary had viewed his treatment of wounds, abscesses, compound fractures, &c., and "not one," he adds, "had ever expressed the slightest doubt that the system in question was *entirely new* . . .!" While I regret the strange and almost incomprehensible want of knowledge with which Mr. Lister charges his professional visitors, I am fortunately not answerable for it, and if Mr. Lister had taken the slightest trouble to search English medical literature alone, he would have easily convinced himself of his own grave error in this respect.' He then proceeds to point out that carbolic acid had been extensively used in surgical treatment before Mr. Lister published, and that Lemaire and Déclat had also understood the principles on which it acted. Had Sir James Simpson underlined the word *system*, and not the words *entirely new*, and had he continued the quotation which runs as follows, 'the novelty, I may remark, being not the surgical use of carbolic acid (which I have never claimed), but the methods of its employment with the view of protecting the reparatory processes from disturbance by external agency,' he could hardly have fallen into such a gross blunder. I need not waste time in refuting Sir James Simpson's accusations.

Several similar mistakes have been committed by other writers; but the only other one which I need mention, as it is also the most virulent attack of all, is that by IGNAZ NEUDÖRFER in 1877.[1] His work is a very strange one indeed. He rejects the germ theory of putrefaction and supposes that the bodies which fall into wounds and cause fermentation are not bacteria, but ferments coming from previously fermenting substances. Strange to say, though holding this view, he rejects also the Listerian method, which one would think would be as natural a

[1] *Die chirurgische Behandlung der Wunden*, 1877.

sequenee of such a view as of the germ theory. He also attempts to show that Lemaire had anticipated Lister in all particulars, but his statements rest on the same fallacious foundation as Sir James Simpson's. He falls into the common error of supposing that Mr. Lister teaches that catgut becomes revivified, instead of, as Mr. Lister has shown, being eaten away and disappearing under the aetion of the neighbouring living tissues; and he is very severe on this subjeet. Indeed here his bitterness against Lister and his teaching erops up in an unwarrantable and most offensive way; and I will quote his statement here as an example of Neudörfer's writings. He says: 'Es ist auch hier, wie bei der Wundbehandlung, nur die Darstellung Lister's welche ganz einfache und klar zu Tage liegende Verhältnisse in ein mysteriöses Dunkel hüllt, so wie das Bestreben Lister's an die Stelle der wissenschaftliehen Erörterung inspirirte Dogmen als Orakel hinzusetzen, welches die Fachgenossen blenden soll, die seinen sonstigen Verdiensten nicht wenig Abbruch thut.'

Mr. Lister's merit in this respect is well stated by Bardeleben in his lectures 'Ueber die Theorie der Wunden.'[1] He says: 'Lange schon wusste man dass ein Magnet Eisen anzieht, lange genug auch, dass ein Eisenstab magnetisch wird, wenn durch einen denselben umkreisenden, aber nicht berührenden Draht ein galvanischer Strom geleitet wird, aber die Anziehung des Eisens durch den vermittelst des galvanischen Stromes magnetisch gemachten Eisenstab für die Telegraphie zu benutzen war doch eine Entdeckung.'—'So steht es auch mit dem segensreichen Fortschritte welchen wir Lister zu danken haben. Joseph Lister hat weder die Bakterien entdeckt, noch die Karbolsäure; aber er hat mit eiserner Konsequenz die Thatsache dass Fäulniss nicht entsteht ohne Einwirkung von Bakterien, oder wie, er um botanischen Spitzfindigkeiten zu entgehen, lieber sagt, von Keimen (und dass solche Keime in der Luft schweben) er hat diese Thatsache angewandt auf den lebenden und namentlich auf den verwundeten menschlichen Körper. Und das Ergebniss dieser Anwendung war *auch eine Entdeckung*, eine Entdeckung wohl werth den grössten Fortschritten der ärztlichen Kunst angereiht zu werden.'

[1] The passage would lose by translation.

Another phase in the history of this subject to which we must allude is the introduction of various modifications.

I have already described the modifications and improvements introduced by Mr. Lister. I have also mentioned the newer methods of *drainage*, more especially the use of absorbable drains introduced by Mr. Chiene, and carried out by him by means of catgut, and by Neuber by decalcified bone tubes.

Neuber has made several attempts to obtain a dressing which may be left on for a long time. The advantage of such a dressing is, of course, evident, more especially in the case of compound fractures, &c., and in country practice. He uses bone tubes as drains, and catgut for stitches. The drains in three or four days become soft, and in about ten days have generally entirely disappeared. Large quantities of gauze (Krüllgaze) are applied next the wound and fastened with a gauze bandage. Outside this comes a mass of salicylic wool soaked in the lotion, also fastened with a gauze bandage, and then outside this the regular carbolised gauze dressing. The first dressing is left on in the case of large wounds for about two days. Then the gauze and the salicylic wool are removed, the deeper gauze dressing soaked with the lotion, and a fresh dressing applied. This may be left on for ten days or a fortnight, provided no discharge appears at the edge. This is one of many ways in which a similar result may be obtained. Of late Neuber has treated several cases with one dressing alone.

With the view of cheapening the materials, various modifications have been introduced: thus Brun's gauze, mentioned before, and also a gauze introduced by Münnich are outcomes of these attempts. Münnich uses glycerine and spirit instead of paraffin. He also in some cases adds stearin. He uses jute instead of gauze. This carbolised jute is much cheaper than carbolised gauze, and has been extensively introduced. Bardeleben has used with great success jute soaked with carbolic lotion and kept moist by frequent addition of lotion. This is covered with impermeable tissue.

Modifications of the method have been made with the view of applying it in *war*, and I have described previously the suggestions of Mr. Lister and Professor Esmarch. A paper will also

be found on this subject by Dr. Burchardt in the 'Deutsche militairärztliche Zeitschrift.'

Various other antiseptics have been introduced as *substitutes for carbolic acid.* Among these I may mention the use of *salicylic acid* by Thiersch. This I have already described. *Thymol* was introduced in 1878 by Dr. Ranke of Halle, and he speaks very highly of its usefulness. It has, however, been tried by several surgeons, among others by Mr. Lister, and has not been found to be a trustworthy antiseptic. Quite recently H. Fischer and Professor Maas have advocated the use of acetate of alumina. Maas uses a 2·5 p. c. spray, and after applying protective to the wound, he covers it with compresses soaked in this solution, and covered with an impermeable tissue. He says that the cases follow an aseptic course, and that, as the antiseptic causes no irritation, the discharge is very slight and the dressings only require to be changed at rare intervals.

Then of late objections have been made to the necessity for the spray in aseptic work, and Trendelenburg [1] has recently published a paper giving the results of some operations performed without the spray. Indeed for four years he has performed all operations strictly aseptically, but in none has he used a spray. The mode in which this has been carried out is that indicated before. During and after the operation the wound is washed out with carbolic lotion and, while the wound contains this lotion, it is closed. During the changing of the dressing Trendelenburg adopts Mr. Lister's former method of allowing fluid to flow more or less constantly over the wound, and more especially over the orifice of the drainage tube. I shall discuss this question at a later period.

Some years ago Mr. Callender published results obtained by a slight modification of Mr. Lister's method, or rather by the use of Mr. Lister's carbolic oil dressings. I shall also allude to these later.

With regard to these attempts, we have already seen that the spray is the least essential detail of the system; and I have described how its use may be done away with with safety. And this brings me to the last point in connection with the history of

[1] *Archiv für Klinische Chirurgie.*

this subject, viz.: that the method has been objected to on various grounds, but chiefly because those who have failed in their attempts have looked on the spray as the essential element, and on aseptic surgery as the performance of operations or dressings in a spray of carbolic acid. There cannot be a more fatal error than this, for the spray is in the great majority of cases merely a convenience, and not a necessity, and those who look on it as a necessity are apt to forget the really essential details, and to trust too much to the spray. I say it is not essential, for if the wound be easily accessible, and be thoroughly washed out during and after the operation, the great probability is that an aseptic result will be obtained. Only it must be remembered that if the spray is not used, this washing out must not be neglected. But to deluge the wound with carbolic lotion is an undesirable thing; and it is for this reason, and also because the feeling of certainty as to the result must be greater when the spray is used than when it is not, that I would advocate its continuance.

Other surgeons look on the gauze as an essential element, and that this is equally an error is evident from a consideration of Mr. Lister's earlier methods, and also from the results at present obtained by the use of other materials. One surgeon has indeed gone so far as to use the term 'spray and gauze method' as synonymous with Listerian or aseptic surgery; in other words, to define aseptic surgery by two of its least or even non-essential elements. Aseptic surgery is not treatment by spray, nor by gauze, nor by spray and gauze, nor by carbolic acid, but is any method of treatment which aims at and succeeds in *excluding* the causes of fermentations from wounds.

CHAPTER XVII.

RESULTS OF ANTISEPTIC SURGERY.

Chief points for consideration. How far do the various methods prevent fermentations in wounds? How to ascertain the true value of any individual method. The value of the various methods in guarding against infective disease: meaning of the term 'infective disease.' Relations of aseptic surgery to infective disease. *Mr. Lister's results in Glasgow. The recent Glasgow statistics. Mr. Lister's results in Edinburgh; results in septic and aseptic cases—Mr. Spence's practice—Mr. Syme's results—Mr. Lister's results at King's College Hospital:—Volkmann—Nussbaum—Socin—Saxtorph—Esmarch—Hueter—Czerny—Lucas-Championnière—Gross—Létiévant—Panas—Schede—Reyher—Spencer Wells—Keith—Thornton. Thiersch's results with salicylic acid. Thymol.* The relations of other forms of Antiseptic Surgery to these diseases—*Treatment by Antiseptics—Reyher—Lister—Nélaton—Hutchinson. Occlusion—Jules Guérin—Alphonse Guérin. Treatment by irrigation and water bath—Langenbeck—Valette. Open Method—Bartscher and Vezin—Burow—Krönlein.* Results where no antiseptic measures were adopted—*Billroth—Malgaigne—Paul—Holmes—Erichsen.* Results of cleanliness—*Savory—McVail—Bardenheuer.*

In looking at the results of the various methods of treatment, the following are the chief points which present themselves for consideration.

1. Results of the various methods as to saving life
   (*a*) by preventing infective disease.
   (*b*) by preventing profuse suppurations and consequent exhaustion.
   (*c*) in other ways, such as by rendering operations on weak or diseased individuals possible, or by enabling the surgeon to undertake with safety by one method operations which by other methods would be unjustifiable.

2. Results according as one or other method enables the surgeon to render the patient a more useful member of society.

Here I shall have chiefly to consider the bearing of the various methods on conservative surgery.

3. Method of healing in various cases, and also the behaviour of sloughs, ligatures, blood clots, &c.

4. The bearing of the various methods on constitutional disturbance after operations or wounds.

5. Discussion of the objections against the various methods, and other points.

Before entering on these questions, I must say a few words as to how far these methods answer the purpose of preventing fermentations in wounds.

The aseptic method, when properly carried out, does this completely. There is no putrefaction, and no other kind of fermentation; and, as I have shown, organisms can be entirely excluded if it be wished. No doubt in ordinary practice one form of organism does sometimes get in, but this is only generally towards the end of the case, and it does not cause putrefaction, while the products of its growth seem to be but little irritating.

Treatment by antiseptics does not prevent fermentations or the entrance of organisms, unless, indeed, it is so thorough as to render the wound aseptic. The specimens of bacteria which are figured on Plate I. were taken from wounds treated with antiseptics. Although, however, fermentations are not altogether prevented, yet from the frequent removal of the organisms and their destruction by the antiseptics employed, fermentation does not, as a rule, occur to any great extent.

The open method hinders the putrefactive fermentation, more especially because the discharge flows rapidly away and also because it becomes too concentrated and too freely admixed with oxygen. Nevertheless, whenever the discharges are retained, they undergo fermentation, showing that the causes of fermentation are constantly present.

Treatment by irrigation or by water bath is more effectual than the open method, because the discharge is removed still more rapidly and thoroughly; but, nevertheless, unless a sufficiently strong antiseptic solution be employed, the causes of fermentation are always present in the wound, and may act if they have opportunity.

The various methods of occlusion are the most imperfect of all, and I have already referred to the stench and the state of the pus in wounds treated by Alphonse Guérin's cotton-wool method.

In determining whether any instrument or any method is safe and suitable for use in the ordinary circumstances in which it will be employed, one does not simply content oneself with using it under such circumstances, but various tests are applied to it, *i.e.* it is subjected to trials greater than those which it will have to undergo in every-day work. A gunmaker is not satisfied with a gun if it does not burst with an ordinary charge; he overcharges it, and if it withstands this test, he very properly concludes that it will be efficient and safe as ordinarily used. A boilermaker does not send out a boiler as trustworthy till he has subjected it to pressure such as it may never have to bear afterwards, but which it might, under rare circumstances, have to endure.

In considering the value of the various methods of wound treatment as protectors against death, we must in like manner consider how they behave in the very worst circumstances, in circumstances in which they may never or only very rarely have to be employed; for a method which is effectual under unfavourable circumstances ought to be employed in all, unless there is some special and valid objection to its use. The tests, therefore, which I shall apply to these methods (so far as I can find the requisite material) with the view of determining how far one or other may be *depended on* as a guard against the more serious dangers of operations, are their behaviour in unfavourable hygienic conditions, or indeed, in infected atmospheres, and to what extent they protect patients after operations which are peculiarly liable to be followed by serious consequences.

Firstly, then, with regard to *Infective Disease.* Under this heading I include Pyæmia, Septicæmia, Erysipelas and Hospital Gangrene. Closely allied to these is Septic Intoxication, which I would call, after Matthews Duncan, Sapræmia. For though this is merely the result of a chemical poison, it is a disease which is dependent for its occurrence on fermentation in

wounds, and it must, therefore, be included in this group. I do not add Tetanus, because I do not think that there is sufficient evidence as yet to justify us in classing it among septic diseases, though several eminent surgeons, more especially in Germany, hold that view.

I shall first consider

THE RELATIONS OF ASEPTIC SURGERY TO THESE DISEASES.

The first record which I can find—and it is a very striking one—is a paper published by Mr. Lister himself, in which he gives his results in Glasgow up to the time when he went to Edinburgh. This will be found in the 'Lancet' for 1870. In this paper he describes the progress of his cases and the state of his wards in the Glasgow Infirmary before and after the introduction of aseptic surgery. He tells us that on account of the constant presence of infective diseases in that hospital when he went to Glasgow, he had to diminish the number of beds in each ward, and he states that infective disease was so common, that whenever a case of compound fracture was admitted into his wards, he at once ordered the internal administration of sulphites, which were at that time much used as prophylactics against these diseases. In some of the other wards these diseases became at times so prevalent that the wards had to be closed.

Mr. Lister gives the following statistical table of his results in amputation cases before and after the introduction of aseptic surgery. The statistics *before* the introduction of that method include the results of two years' practice (1864 and 1866, the report for 1865 being imperfect). During that time the following amputations were performed, with the following results:—

	Cases	Deaths
Through the shoulder-joint	3	2
,, upper arm	3	2
,, elbow-joint	1	1
,, forearm	5	1
,, hip	5	4
,, thigh	10	3
,, knee-joint	3	1
,, ankle	5	2
Total	35	16

A mortality of 45·7 p. c.

The causes of death are not definitely stated, but almost all were due to infective disease. Thus of the six deaths following amputations of the upper extremity, four were due to pyæmia and one to hospital gangrene.

In contrast to this, Mr. Lister mentions the results obtained during the aseptic period (1867, 1868, 1869). The following were the amputations performed :—

	Cases	Deaths
Shoulder-joint	3	—
Upper-arm	3	
Forearm	6	1
Hip-joint	2	1
Thigh	4	
Knee	13	4
Ankle	9	
Total	40	6

A mortality of 15 p. c.

The causes of death in all the cases is not given. In two—an amputation at the hip-joint and a double amputation at the knee—death occurred from shock and loss of blood. Mr. Lister also states that two of the deaths resulted from pyæmia, but in one of these the pyæmia existed before the operation (amputation of the forearm), and in only one case (an amputation at the knee) did pyæmia arise after the operation. But further, these were the only cases of pyæmia which occurred in Mr. Lister's hospital practice during these three years, and that in spite of the former frequency of the disease. And among the other cases treated during this time were twenty-two compound fractures, and several compound dislocations.

During the same period only one case of erysipelas occurred, but it did not prove fatal.

Hospital gangrene attacked one or two wounds, but it was of a very mild type; and Mr. Lister states that during the last year of this period he had no cases of that disease.

Two other points must also be taken into consideration. Firstly, the annual cleaning of the wards, which had previously been necessary, was not carried out during those three years. Secondly, when Mr. Lister remarked the great improvement in the healthiness of his wards, he increased the number of beds in each.

Here we have a very valuable piece of statistics; and it is so much the more striking as the results were obtained in the early days of aseptic surgery before any of the recent improvements had been introduced. During a period of three years, there occurred only two cases of pyæmia, one case of erysipelas and one or two cases of mild hospital gangrene, and this result was obtained in unhealthy wards in which these diseases were previously common, and in wards which, towards the end of the aseptic period, were in much more unfavourable hygienic conditions than before the commencement of that period. Looking at only one class of operations, the mortality after amputations was reduced from 45·7 p.c. to 15 p.c., and that reduction was in the main or altogether owing to the abolition of infective disease. I say that these statistics are of the greatest value, and it is strange that they have been allowed to pass unnoticed by those who have cried so loudly for statistics; for they answer almost all the requirements. They are results obtained by the *same* surgeon in the *same* wards in successive years. And the result cannot be ascribed to better hygienic conditions; for, as I have just pointed out, there was no improvement in the hygienic arrangements during the aseptic period, in fact, rather the contrary, for the annual ward cleaning was done away with, while an increased number of beds was introduced. Nor can the results be ascribed, as is so much the fashion in some quarters, to cleanliness alone, to the cleansing of the instruments after each dressing, &c.; for long before Mr. Lister had thought of the aseptic method, he had striven to prevent all such contamination, being fully impressed with the evils of putrefaction, and with the necessity of avoiding it, as far as possible. Nevertheless, though cleanliness had been tried, it had failed.

While speaking of Glasgow, I may mention here the recent statistics given by Mr. MacEwen in 1879.[1] They are merely numerical, but nevertheless, they are striking, and when considered along with the other results which will be referred to presently, they will be seen to be of great value. Mr. MacEwen, thinking that pure air was of more value than aseptic treatment, asked the medical Superintendent of the Glasgow Infirmary to compile comparative statistics of cases treated asep-

[1] *British Medical Journal*, 1879.

tically and of those treated in other ways. To his surprise, the facts showed the opposite of what he had expected. Thus, during the years 1875, 1876, 1877 and 1878, 1706 cases were treated strictly aseptically under the care of Dr. Cameron, and of these 50 or 2·93 p.c. died. During the same period, in the same number of wards, under Dr. Morton's care, 1884 cases were treated not aseptically, and of these 110 or 5·84 p.c. died, the cases in each instance being practically of the same character. And not only was the mortality in the latter case more (it was double) than in the former for the whole period taken together, but a similar difference was found when the results of each year were looked at separately. Thus the percentage mortality during

1875	was, among	aseptic cases	3·29 p. c.
,,	,,	non-aseptic cases	7·63 ,,
1876	,,	aseptic cases	3·28 ,,
,,	,,	non-aseptic cases	6·91 ,,
1877	,,	aseptic cases	3·68 ,,
,,	,,	non-aseptic cases	5·13 ,,
1878	,,	aseptic cases	2·93 ,,
,,	,,	non-aseptic cases	3·96 ,,

As is remarked in a leader in the 'British Medical Journal' on this subject: 'The result, therefore, proves that, under antiseptic treatment, the mortality was, under apparently strictly comparable circumstances, much smaller than under the ordinary mode. . . . This is a case very much to the point, and will meet some of the conditions of comparative statistics rightly insisted on by Mr. Holmes.' Some correspondence followed between Dr. Cameron, Dr. Morton, and Mr. MacEwen, after the publication of these results, but the further information elicited did not in any way alter the significance of the facts.

I will now pass on to Mr. Lister's results in the Edinburgh Infirmary. These have been already published in his speech at the meeting of the metropolitan branch of the British Medical Association at St. Thomas's Hospital in December, 1879;[1] and some further facts were given by Mr. Lister in his reply, in February 1880, to Mr. Spence's attack.[2] I shall not, therefore, enter at great length into these general statistics, more espe-

[1] See MacCormac's *Antiseptic Surgery*.
[2] *British Medical Journal*, 1880.

cially as I intend presently to allude to some of the facts in detail.

From the end of 1871 to the middle of 1877—a period of about five and a half years—Mr. Lister treated 553 cases aseptically. Of these 2, or ·36 per cent., died of blood poisoning. During the same period, Mr. Lister treated 292 cases in other ways, some with antiseptics, some without, and of these 4, or 1·36 p. c., died of blood poisoning. Now this alone is a very striking statistical fact, as it shows that the same surgeon, in the same wards, during the same time, lost four times as many patients from blood poisoning in cases not treated aseptically as in those which were treated on strict aseptic principles. And when we look at the nature of the cases in each instance, this difference will become much more apparent.

Of the 553 cases treated aseptically, 29 died.

Among these were 80 major amputations, of which 9 died—6 from shock within a few hours, 1 from diphtheria in the throat nine weeks after operation, when the wound was almost entirely healed; 1 from cerebral hæmorrhage three months after the operation; and 1 from hæmorrhage into the thigh from a malignant tumour of the femur three days after amputation at the shoulder-joint—the amputation wound was doing well.

The following is a complete table of Mr. Lister's amputations :—

	Primary		For disease and secondary to injury		Total	
	Cases	Deaths	Cases	Deaths	Cases	Deaths
Hip	1	1*	2	2*	3	3
Thigh	3	2*	26	1†	29	3
Leg	3	0	5	0	8	0
Ankle	2	0	16	1‡	18	1
Shoulder	4	1*	1	1§	5	2
Upper-arm	3	0	6	0	9	0
Forearm	—	—	8	0	8	0
Total	16	4	64	5	80	9

* Died in a few hours from shock; † died from diphtheria; ‡ from cerebral hæmorrhage; § from hæmorrhage into the thigh.

There were also 21 excisions of the larger joints performed asep-

tically without a death (7 of knee, 2 of shoulder, 10 of elbow, 2 of wrist).

Thirty-seven excisions of the mamma were performed aseptically with two deaths, both from infective disease. One of the deaths was from septicæmia occurring after the removal of a very large portion of skin and of the whole contents of the axilla. Everything went on perfectly till the tenth day, when a mistake was committed in the dressing; putrefaction occurred, and septicæmia commenced. On post-mortem examination no abscesses or infarcts or other marked appearances were found. The other patient died of erysipelas. Among these 37 cases there were 24 in which not only the mamma, but also the fat and glands from the axilla were removed.

There were 27 operations for un-united, or badly united, fractures without a death. These consisted of 8 operations on the femur, 9 on the bones of the leg, 4 on the humerus, 5 on the bones of the fore-arm, and 1 on the clavicle (removal of a projecting splinter of bone in a case of simple fracture, thus converting the case into one of compound fracture).

There were 14 operations on healthy joints, in which the joints were opened and kept open for some days by means of a drainage tube. No death.

In 11 cases incisions were made into diseased joints in which suppuration had not yet occurred. No death. These were cases of gelatinous disease.

There were 39 cases in which abscesses of joints were opened and a drainage tube inserted, none of the diseased parts (bone, synovial membrane, &c.), being removed. Of these, 2 died, both from tubercular meningitis confirmed on post-mortem examination.

An incision was made and a drainage tube inserted in 2 cases of synovitis of the knee-joint. No death.

There were 4 cases in which operations were performed to relieve old standing dislocations. In some of these the bones were simply replaced. In one case it was necessary to remove portions of the bone before the surfaces of the joint could be brought into apposition. No death.

In 3 cases the femur was divided for knock-knee. No death.

In 7 cases of ostitis a groove was dug in the bone by a gouge and hammer. No death.

There were 7 cases of ligature of the large arteries in their continuity, with 1 death. The fatal case is not entered in the note-books, but I remember its occurrence. During attempts to reduce a dislocation of the shoulder-joint of seven weeks' standing, the axillary artery

was torn and hæmorrhage occurred into the axilla. Mr. Lister at once cut down and ligatured the artery, but the patient died the same night from exhaustion, owing to the loss of a large quantity of blood.

There were 2 cases of excision of the thyroid gland. No death.

There were 4 cases of excision of the testicle. No death.

There were 9 cases of strangulated hernia, with 3 deaths. The gut was gangrenous in all the fatal cases.

There were 30 cases of abscess (psoas or lumbar) connected with disease of the spine. Of these 4 died—2 from phthisis, 1 from exhaustion, and in one case the lumbar abscess was almost absolutely healed when a little glandular abscess formed in the neck. This was opened without aseptic precautions, and the wound was attacked with erysipelas, of which the patient died. Though this was an aseptic case, yet the erysipelas did not attack a wound treated aseptically, and therefore the death from infective disease is included among the cases not treated aseptically, and not among the aseptic ones.

There were 91 cases of acute or chronic abscesses. These included a great variety of abscesses, but they were all more or less extensive. There were 2 deaths; one death occurred in a case of peri-renal abscess. The patient was in a very low state when operated on, had diarrhœa, &c., and he sank in ten days. On post-mortem examination the typical scrofulous kidney was found. The other death occurred in a case of abscess in the right lumbar region, but in which there was no disease of the spine. The abscess was opened on May 20, 1873, and went on very well till July. There was then only a small sinus, and the patient was permitted to get up. During autumn the discharge was allowed to putrefy, and it afterwards increased rapidly in amount and became purulent. For some days before her death, on November 15th, the patient had very severe pain in the right iliac region and the right limb, and following this the leg and foot became gangrenous (phlebitis?).

There were five cases of empyema. No death.

There were eight cases of chronic bursitis, in which incisions were made and drainage tubes inserted. No bad result.

There were twelve cases of removal of exostoses without a death.

There were forty cases of removal of large tumours from various regions. No death. (There were also a number of cases in which smallish tumours, fatty or otherwise, were removed. These are not included among the forty.)

There were three cases of suprapubic lithotomy, with two deaths. In one fatal case, an adult male, the peritoneum was intentionally opened below the umbilicus, the bladder incised through its peritoneal coat,

and the stone extracted. The wound in the bladder was then stitched up, as was also the wound in the abdominal wall. The patient was doing well till the second morning (about forty hours) after the operation, when he got out of bed, or was made to get up. As a result of this exertion the intestines protruded between the stitches. They were returned as soon as the accident was discovered, but the patient died of shock on the same day. In the other fatal case, a little boy, the peritoneum was also opened, but the stone was not removed. The child cried incessantly after the operation, and in spite of the close stitching of the wound in the abdominal wall, the intestines escaped between the stitches. The patient died on the following day from shock.

There were three cases of spina bifida, into which minute drains, in two cases horse-hair (two or three threads), and in one a drainage tube, were introduced. The patients apparently died as the result of the constant draining away of the cerebro-spinal fluid, although as soon as bad symptoms were evident, the drains were withdrawn. In two cases no macroscopical appearances were found to account for death. (I do not know what the microscopical appearances of the cord may have been.) In one case the sac was congested, and contained a little turbid fluid, but there was no violent inflammation, and the little which was present did not apparently spread up the canal. In fact, the appearances found could not explain the fatal result in any of the cases, and therefore it is possible that the disturbance due to the constant draining away of the cerebro-spinal fluid had something to do with death. The first two cases died in two and nine days respectively. The last died in five days.

One case of chronic hydrocephalus was treated in this way (by drainage) and died in six days. There was no trace whatever of inflammation. The ventricles were extremely distended, containing forty-two ounces of clear cerebro-spinal fluid. Apparently the fatal result was due to the same cause as in the former instances, viz., the disturbance consequent on the constant draining away of the cerebro-spinal fluid.

These are the most important of the 553 cases. (Although there were only two deaths from infective disease after aseptic operations, and although that fact was all that was necessary for our present purpose, I have thought it well to mention all the causes of death, and to indicate the sort of cases treated.)

The 292 cases not treated aseptically contained a very much larger proportion of trivial operations, such as removal of

necrosed bone, fistula in ano, hæmorrhoids, &c. There were ten deaths, of which two were from pyæmia, one from septicæmia, one from erysipelas, and six from exhaustion (?).

The cases of pyæmia occurred, one after a plastic operation on the nose, and one after amputation of the penis. The case of septicæmia occurred after excision of the tongue. The case of erysipelas occurred after the opening of a small abscess in the neck without aseptic precautions; this case is mentioned among the psoas abscesses (p. 373). One case of excision of the hip-joint (æt. seventeen) died twenty-nine days after the operation, apparently of exhaustion from the profuse discharge. One case of excision of the mamma (æt. sixty-three), where putrid sinuses were present before the operation, died in three days of exhaustion. (May not the fatal result in this case have been due to sapræmia ?) One case of excision of the upper jaw (æt. sixty-three), died twenty-three days later, apparently of exhaustion; there were no marked symptoms during life, and no post-mortem appearances; there were hæmorrhages on various occasions after the operation. One case of excision of the tongue (æt. seventy-five) died in ten days without any special symptoms, apparently of exhaustion; no morbid appearances were found on post-mortem examination. One case in which the floor of the mouth was removed for malignant disease (an old woman) died apparently of exhaustion. One case of old standing, necrosis of the ilium (æt. twenty), died twenty-three days after an attempt to remove the dead bone. He is also said to have died of exhaustion, but there is no record of the post-mortem examination, and he had coffee-ground vomiting during the forty-eight hours preceding death. (Several of these cases of exhaustion were probably cases of sapræmia or septicæmia.)

If we compare the causes of death in the two instances and the nature of the operations, the case in favour of the aseptic method becomes much stronger than if we simply compare the deaths from infective disease. In the septic cases the patients either died of septic disease or of exhaustion, the result of profuse suppuration, again the result of putrefaction. If we consider the aseptic cases, on the other hand, we get a very different result. Thus, among the deaths after amputation there was not a single case where any method of treatment applied to the wound could possibly have saved the patient. The two deaths from tubercular meningitis, the three cases of hernia in which the gut was gangrenous, and the two cases of phthisis were all independent of the treatment of the wound.

The case of peri-renal abscess was also hopeless, and so also, possibly, the case of abscess in the loin where gangrene of the foot occurred. In the case of exhaustion after psoas abscess the disease was extremely extensive. But suppose we include this case, the two cases of infective disease, the three cases of spina bifida, the case of hydrocephalus, and the two cases of suprapubic lithotomy, we have only nine instances of what we may term preventible deaths. In the last six cases, however, it was rather to the direct surgical interference than to anything in the after-progress of the wound, looked at from our present point of view, that the deaths must be attributed; and here, of course, we are not considering the former point. Leaving then out of view the question of the surgical interference, there are only three cases among these, which can be considered in connection with the method of wound treatment alone. Indeed, I doubt very much if it is fair to include the case of exhaustion after psoas abscess, for the disease was of such a nature, so very extensive, as to render it doubtful whether recovery was possible under any circumstances.[1]

On the other hand, among the fatal septic cases, there were none independent of the after-progress of the operation wound, unless indeed we exclude the case of necrosis of the ilium in which probably waxy degeneration of the internal organs had occurred extensively before the operation. This leaves 9 out of 292 septic cases, of which a large proportion were trivial operations in which death occurred on account of the course which the wound followed, while in the former instance, in the 553 aseptic cases, a very small proportion of which were trivial operations, there were only, at the most, three such instances.

During this same period there were treated aseptically in hospital seventy-two cases of injury (wounds, compound fractures, and wounds of joints), of which four died.

Three of these deaths occurred in cases of compound fracture treated conservatively, and in all death took place within 48 hours. The other death occurred in a case in which the attempt to eradicate putrefaction was unsuccessful, and where the patient is said to have died of bronchitis and cardiac disease.

[1] See the list of psoas abscesses, No. 21, Chapter XX.

Here again, there is only one case of possibly preventible death, and that in a septic case. Among those which were rendered aseptic no death occurred which was preventible by any known method of wound treatment.

We have fortunately the opportunity of comparing these results with those obtained by another surgeon—Mr. Spence—in the same hospital during the same time, by the use of methods of treatment which were not aseptic but which consisted sometimes in the application of water dressing, sometimes of boracic lint, and in some cases no dressing was used. I cannot give a name to the method of treatment. It was a mixture of principles, in fact a sort of mongrel method. There were certain differences in the hygienic conditions which will be presently alluded to, but these were all more favourable to Mr. Spence.

Mr. Spence's report extends from October 1872 to April 1878 with the exception of the winter session 1874–75.[1] I regret that I cannot find any report for this session, as during it there was a virulent epidemic of erysipelas in Edinburgh, and it would have been interesting to know how Mr. Spence's cases progressed during that time. Taking, however, the period so far as it is given, but always remembering that a very testing winter included in Mr. Lister's results is not present here, we find that during this time 328 more or less severe operations were performed with fifty-eight deaths, and that three cases of compound fracture were treated conservatively with one death. What proportion of these deaths were due to infective disease, we shall, I suppose, never know. In sixteen out of the fifty-eight cases the cause of death is not stated at all; five died within thirty hours, and may therefore be left out of consideration; while in eight the fatal result is directly assigned to septic poisoning. In twenty-nine of the cases no distinct causes of death are given, though such statements as the existence of irritative fever, unhealthy action in the wound, uncontrollable oozing of blood, &c., lead us to suspect that infective disease was also at work in these instances.

These results form a marked contrast to Mr. Lister's.

[1] See *Medical Times and Gazette*, March 13, 1875; the same journal for October 28, 1876; and the November and December numbers of the *Edinburgh Medical Journal* for 1879.

Here we have a percentage mortality of nearly eighteen p.c. as compared with Mr. Lister's percentage mortality in aseptic cases of nearly five p.c., i.e. a mortality in septic cases 3·7 times greater than in aseptic ones. And further the cases of death which are distinctly stated by Mr. Spence as having been due to septic diseases were eight in number or 2·4 p.c., that is to say, eight times more than the mortality from similar causes in Mr. Lister's aseptic practice. And, as I have just indicated, the mortality from these causes in Mr. Spence's practice was probably very much greater; and this difference is not due to greater severity of the operations in Mr. Spence's practice, as we shall see from the following statement of the nature of his cases.

During this period Mr. Spence performed 97 amputations, of which 25, or 25·7 per cent., died. (Compare this with Mr. Lister's mortality of 11·25 per cent. after amputations performed on aseptic principles.)

The following table gives Mr. Spence's results in major amputations:—

	Primary		Secondary to injury and for disease		Total	
	Cases	Deaths	Cases	Deaths	Cases	Deaths
Hip	0	0	3	1	3	1
Thigh	5	3	26	6	31	9
Knee-joint	1	0	0	0	1	0
Leg	4	0	9	2	13	2
Ankle	1	0	24	2	25	2
Shoulder	1	1	3	1	4	2
Arm	2	1	7	2	9	3
Fore-arm	2	2	2	2	4	4
Total . .	16	7	74	16	90	23

From this table I have excluded 6 partial amputations of the foot, with 2 deaths, and 1 partial amputation of the hand. These are included by Mr. Spence among his major amputations, but I have already excluded such operations from Mr. Lister's list.

As to the causes of death in these cases, one patient on whom amputation at the hip-joint was performed rallied after the operation, but sank suddenly next day. One case of primary amputation of the thigh died from shock, and 1 from pyæmia: 1 amputation of the

thigh for disease died of tubercular meningitis; 1 died 5 days after the operation, and it is stated that he had diarrhœa; in 1 case oozing of blood came on 2 or 3 days after the operation. One secondary amputation at the ankle-joint died from septic poisoning, and 1 had diarrhœa and tabes mesenterica. One fatal case of amputation at the shoulder-joint had repeated hæmorrhages from the wound. One case of amputation through the upper arm died 2 days after the operation, having had continuous oozing of blood; in 1 case general oozing began on the 6th day but was arrested by styptics, and the patient died on the 7th day. One primary amputation of the fore-arm died from pneumonia, and 1 had fever, traumatic delirium, diarrhœa and vomiting, and died on the 10th day; 1 died from erysipelas and 1 from pleurisy and pneumonia. The causes of death in the other fatal cases (8 in number) are not referred to.

Among the other cases treated were 57 excisions of various joints, of which 7 died; 34 excisions of the mamma with 2 deaths; 10 cases of lithotomy with 1 death; 44 cases of removal of tumours with 8 deaths; 9 complete or partial excisions of the tongue, 4 deaths; 4 cases of tracheotomy, no death; 31 operations for necrosis, no death; 1 operation for badly-united fracture of the femur, no death; several miscellaneous severe operations, 6 deaths; 3 excisions of the lower jaw, no death; 3 castrations, no death; 2 cases of trephining.

On comparing these cases with Mr. Lister's we find that the operations were not more severe, and yet the mortality was nearly four times as great.

Mr. Spence had thirty-one amputations of the thigh against Mr. Lister's twenty-nine, but nine deaths against three.

The total number of cases in which bones were operated on, such as for ununited fractures, excisions, &c. was greater in Mr. Lister's practice, and yet he had no death as compared with Mr. Spence's seven fatal cases. This difference is the more remarkable when it is remembered that many of Mr. Lister's operations were performed on healthy bones, while the greater number of Mr. Spence's were performed on diseased bones. It is less dangerous to operate on the extremity of a bone than on the dense shaft, especially where the medullary cavity is opened. And the dangers of the operation are diminished if the bone be previously the seat of chronic inflammation, for a chronically inflamed bone is well advanced on the road to

granulation, and granulation is, therefore, much sooner complete after the operation than in the case of healthy bone. In other words, the time during which there is risk of absorption from the wound is shorter in the case of chronically inflamed bone than in the case of healthy bone, while the risk is from the first less, because in the diseased bone a partial barrier is present from the beginning.

Then, among Mr. Spence's cases there is not a single instance of simple incision and insertion of a drainage tube into a healthy joint, a diseased joint or a suppurating joint, while Mr. Lister had seventy-six of these cases.

Then, also, Mr. Spence does not seem to have treated a single case of psoas or lumbar abscess.

Thus, however we look at the matter, the contrast between septic and aseptic surgery is very markedly in favour of the latter. This is well seen in Mr. Lister's own hands, in which the results of cases treated aseptically were much better than of those treated in other ways, these results being obtained by the same surgeon, in the same wards, and during the same time. The same contrast in favour of aseptic surgery is shown by the results obtained by another surgeon, in the same hospital, during the same time, and under circumstances in every way more favourable, both as regards the severity of the cases and the hygienic conditions under which the patients were placed. With regard to the latter point, it must be noted that Mr. Spence's wards were well ventilated and at the top of the building; Mr. Lister's wards were at the lower part of the building, some on the basement floor. Mr. Spence did not overcrowd his wards; Mr. Lister had as a rule nearly seventy patients in wards containing fifty beds, and these beds were more closely packed than Mr. Spence's. (The way in which this was managed was, that those adult patients who were well enough to be up during the day, slept on mattresses placed in various parts of the ward at night, while children were placed two, three and sometimes even four in a bed.) Then, lastly, Mr. Spence's wards were thoroughly cleaned out once a year; Mr. Lister's wards, on the other hand, did not, at his own request, undergo this annual process.

That the wards in which Mr. Lister worked were unhealthy

wards, is shown by the results obtained by Mr. Syme during his last four years. Of amputation cases (120 in number) he lost twenty-five from infective diseases alone, while Reyher,[1] who published these comparative statistics, states that Mr. Lister, up till the end of 1873, in 123 amputations, had not lost one from infective disease, unless indeed we include a case of tetanus as such.

These statistical facts fulfil the conditions required by Mr. Holmes in his recent utterances, and they are therefore deserving of careful consideration.

I shall now pass on to consider Mr. Lister's results in King's College Hospital, from November 1877 till November 1880. During this period there were 207 operations performed aseptically, of which fourteen died. All of the operations were more or less serious, and the following is a sample of them, with the causes of death in the fourteen cases.

There were 3 amputations at the hip-joint for disease, with 1 death. This case died from shock half an-hour after the operation.

There were 4 amputations of the thigh for disease, of which 3 died :—1 got suppression of urine on the second day and died ; his kidneys were extremely fatty, and the fact that he had marked albuminuria had been overlooked before the operation. One case died of pneumonia ; he had a slight cold before the operation, caught from draughts in the ward ; afterwards he got pneumonia, which was apparently distinctly traceable to exposure during the energetic ventilation of the wards in the intense cold of winter. (Such ventilation is, I need hardly say, unnecessary in wards where aseptic treatment is carried out ; the comfort of the patients is the point to be attended to rather than the constant flow of a current of air through the wards.) A post-mortem examination was not allowed in this case The third case was one of spontaneous gangrene of the foot and leg, where amputation of the thigh was performed. Gangrene recurred, and the patient died on the 4th day after the operation.

There were 2 amputations of the fore-arm. No death.

There were 16 excisions of the mamma and axillary glands, with 2 deaths. Both operations were very extensive, and both patients died from shock within 36 hours.

There were 13 incisions into healthy joints, no death ; 5 incisions into joints affected with synovial degeneration, but without suppuration, no death ; 3 incisions into joints affected with synovitis.

[1] *Archiv für Klin. Chirurgie*, Bd. xvii. p. 499.

no death; 9 incisions into suppurating joints, 2 deaths, one of which was due to tubercular meningitis, confirmed on post-mortem examination, and 1 occurred in a child admitted with acute abscesses in various joints, in fact with pyæmia after scarlet fever; he died 2 days after admission.

There were 31 operations on healthy bones for deformities, ununited fracture, &c. No death.

There were 8 abscesses (lumbar or psoas) connected with disease of the spine. One death. This case putrefied, and the patient was sent home, but died at home from exhaustion a few weeks later. Though not a death in hospital, it is a death in a case treated at the first aseptically, and I have therefore included it. Putrefaction occurred owing to the circumstance that the patient suffered from carbolic acid poisoning, and hence the dressings were left on longer than usual, and thymol dressings were ultimately substituted for the carbolic gauze, with the result that the discharge putrefied.

There were 4 cases of strangulated hernia with 3 deaths. In one fatal case the bowel was gangrenous; the gangrenous portion was cut away and the divided ends of the gut united by suture; the patient, who was collapsed at the time of the operation, died in 2 or 3 hours. In one case the patient, an old woman, was in a state of collapse at the time of the operation and never recovered, dying within 24 hours. In one case the gut, which was in a suspicious state at the time of the operation, gave way 60 hours later, and its contents escaped into the peritoneal cavity. The patient died in a few hours.

There were 3 operations for radical cure of hernia, in which the greater part of the sac was cut away and the margins stitched with cat-gut. No death.

There were 3 cases of acute necrosis where incisions were made down to the bone and drainage tubes inserted; the bones were not resected, as Mr. Spence recommends, such a procedure being unnecessary with aseptic management. One of these cases, a child, which was apparently in a septicæmic state before the operation, died 5 days afterwards.

There were 23 large abscesses, among which were 6 iliac abscesses, 3 gluteal, 1 pelvic, 2 mammary, 2 in calf, 2 in thigh, 1 case in which there were 7 chronic abscesses, 2 cases of suppuration of bruises; all of them extensive. No death. Two cases of empyema. No death.

There were 2 cysts of the thyroid gland, which were opened and drained; no death. Three excisions of the thyroid gland; one death. In the fatal case the tumour was adherent to the trachea,

which had become thinned by the pressure, and in removing the tumour the thin membrane gave way. Thus the case was no longer an aseptic one. The putrid matters passed down the trachea, and gave rise to inflammation of the bronchi. The difficulty in breathing was not relieved by the operation. The patient died 16 days after the operation. At the post-mortem examination an opening was found in the trachea large enough to admit the tip of the little finger, and communicating with the wound. The lungs were œdematous, and a small quantity of pus could be squeezed from various parts, but there were no abscesses or infarcts. A large tumour was found in the thorax surrounding the trachea, and partly also the arch of the aorta. There were no other post-mortem appearances. This is, of course, a septic case, and ought not to be included in this list, but I insert it, as the operation was undertaken with the intention of performing it aseptically, and as it is such a marked contrast to the others.

Then there were a number of single operations, which it would be too tedious to enumerate, such as 2 cases of nerve stretching, 2 castrations, 1 operation for aneurismal varix, 5 varicoceles (veins tied), removal of tumours of various kinds, &c.

If now we look at the causes of death we find that several were unconnected with the treatment of the wound. Thus the case of shock after amputation at the hip-joint and the two fatal cases after excision of the mamma are quite irrelevant. So also are the three deaths after strangulated hernia, the death from tubercular meningitis, and the death of the little boy admitted with pyæmia.

How are we to look at the cases of death from suppression of urine, after acute necrosis, and after recurrence of gangrene? Could these have been prevented by any method of treatment? The thyroid case was a septic one.

This leaves us with two cases to consider: the case of pneumonia, which I myself saw, and which I, as well as others who know the facts, firmly believe to have been caused by imprudent ventilation; and the fatal case of psoas abscess, which undoubtedly ought to have been avoided, and would in all probability have been so, had not the patient been so sensitive to the effects of carbolic acid.

During these eight years, three wounds of healthy joints and fourteen compound fractures were treated, with one death.

This death occurred in a case of compound fracture of the skull, the patient being comatose when admitted; trephining was performed, but he died within twenty-four hours. There were also a number of more or less severe wounds under treatment at the same time.

Looking then at the whole results attained by Mr. Lister, we must, I think, come to the conclusion that he is correct in stating, from his own experience, that infective disease is abolished by aseptic treatment, so that, if an operation can be performed aseptically the risk of infective disease may practically be left out of consideration in deciding on the advisability of the operation. This view has been amply borne out, as I have shown, by thirteen years' constant work in three different hospitals—none of them particularly noted for healthiness.

One of the first surgeons to take up the aseptic method thoroughly, was Professor Volkmann of Halle; and he has contributed some remarkable testimony to the efficacy of the system. For many years he had used the open method, and during the war in 1866 all amputation wounds were treated in this way. He also employed immersion in water containing carbolic acid: recent wounds of the hands and feet were placed in vessels containing this solution. In his earlier cases the wounds were stitched, drainage being provided for, but for the four years preceding the adoption of the aseptic method—*i.e.* up till 1872—he left the wounds quite open. During the first years in which these methods of treatment were carried out, the results were very favourable, and Volkmann was thoroughly convinced of their superiority over the older modes. As time went on, however, and as overcrowding of the wards became necessary, infective diseases also progressively increased, and at last, in the summer and autumn of 1871, the deaths from pyæmia and septicæmia were so numerous that he made up his mind to shut up the hospital altogether for a time. Before doing so, however, he thought that he would try the Listerian method for a few weeks, and it is to the result of this trial that I now wish to refer.

I need not go into the details which he gives as to the hygienic conditions of the hospital. They seem to have been very wretched. There were no proper arrangements for ventilation; waterclosets opened into the wards; there was no place for keeping the dead bodies, which were therefore laid in a cellar situated beneath the surgical wards, and the wards were full of beds. It was under these conditions and in this infected atmosphere that the aseptic method was first employed.

Volkmann's first report extends from December 1872 to February 1874.[1] This was the period in which he was learning the method, and I shall not, therefore, refer at length to his results during this time. Among the cases in which the aseptic method was tried there was one death from pyæmia and one from erysipelas. Erysipelas attacked eight wounds treated on aseptic principles. There were other cases of infective diseases in the hospital during this time, but these were either admitted while suffering from them, or they arose in cases not treated aseptically.

This remarkable result was obtained in a hospital which, at the commencement of this period, was going to be closed on account of the enormous mortality from these diseases. Surely here there was some benefit derived from the introduction of the aseptic method! And it must be noted that these two deaths from infective disease occurred in the early period of aseptic practice and, with regard to them and the erysipelas cases, Volkmann states that he could generally point out an error in the manipulations, as a rule, in the mode in which the dressing was applied. In a note written in 1875 Volkmann states, that during the last eighteen months,[2] *i.e.* from the middle of December 1873, there had been no case of pyæmia, and erysipelas had almost or altogether disappeared. With regard to these results Volkmann expressly shows that they were not due to the mere use of carbolic acid as a disinfectant, for carbolic acid had been, as I have just stated, extensively em-

[1] *Beiträge zur Chirurgie*, 1875: 'Ueber den Antiseptischen Occlusiv Verband und seinen Einfluss auf den Heilungsprocess der Wunden;' Volkmann's *Sammlung*, No. 96, 1875.

[2] In the Appendix Volkmann states that this holds true for metastatic pyæmia, but that only fifteen months had elapsed since he had a case of 'Pyæmia simplex.'

ployed during the preceding period to irrigate wounds, but latterly without any apparent benefit.

In 1877 Volkmann published the continuation of this report,[1] and I will refer to it in some detail. The report extends from March 1874 to March 1877, a period of three years. He does not give all the cases treated, but, omitting the septic cases, there is a record of 465 operations performed with aseptic precautions. Of these cases 29 died.

Among these 465 operations were 157 amputations, of which 15 died. On analysing these cases of amputation we find that 139 of them were uncomplicated with other injury or mutilation, and of these only 4 died. Three of these deaths occurred within the first 24 hours from shock, and 1 from 'habitual erysipelas.' Omitting from this list 7 partial amputations of the foot, there were 132 major amputations, with 4 deaths, or, leaving out the cases of shock, which were of course independent of the method of treatment, there were 129 major amputations, with 1 death.

The following is the list arranged in a tabular form :—

	Cases	Deaths
Amputation at the shoulder-joint	4	1*
,, through humerus	14	—
,, ,, forearm	23	—
,, at wrist-joint	3	—
,, ,, hip-joint	2	1*
,, through thigh	42	1*
,, ,, leg	25	1†
,, at ankle (Syme and Pirogoff) .	19	—
,, partial of foot	7	—
Total	139	4

* Died from shock ; † from 'habitual erysipelas.'

There were 9 cases of double amputations, with 2 deaths ; 1 died within a few hours, and 1 died on the third day (amputation through both thighs) with symptoms of collapse.

There were also 6 amputations in cases where other severe injuries had been received. Of these 4 died within 24 hours. These were all very severe cases, as for example, amputation at the shoulder-

[1] *Vorläufiger Bericht über die innerhalb der letzten drei Jahre in der chirurgischen Klinik zu Halle stationär oder poliklinisch mit Hülfe der Antiseptischen Methode behandelten schweren Operationen und schweren Verletzungen.* By Volkmann and Kraske, Halle, 1877.

joint with fracture of sternum and ribs; fracture of the skull, fracture of ribs, and amputation through both thighs, &c. One case died on the fourth day, never having recovered from the state of collapse; here amputation through the thigh had been performed, and the patient had also sustained a severe bruise of the abdomen. One case of amputation of the thigh, along with a severe injury to the hand, died of tetanus on the 14th day.

Lastly, 3 cases died of intercurrent diseases. These were a case of amputation through the leg, which died of delirium tremens; an amputation through the femur, from pneumonia, on the 21st day, the wound being practically healed; and a case of amputation through the humerus, in which abortion occurred, followed by the patient's death from puerperal fever.

Ninety-one excisions of joints were performed, with 5 deaths: 2 excisions of the hip died from shock; 1 excision of the hip from hæmorrhage $3\frac{1}{4}$ months after the operation; 1 excision of the hip from thrombosis of the iliac vein two months after the operation: 1 excision of the knee from tubercular meningitis. Among these cases were 44 excisions of the hip, which recovered. Two cases died from intercurrent disease, viz., 1 excision of ankle-joint from delirium tremens, and 1 excision of the shoulder-joint from phthisis and hæmoptysis. Thus there were in all 93 cases of excision of joints with 7 deaths.

There were 10 operations for ununited or badly united fracture without a death. Also 50 cases of osteotomy, of which 1, a patient affected with the hæmorrhagic diathesis, died from hæmorrhage.

There were 45 cases of hydrocele treated by opening the sac, stitching it to the skin and introducing a drainage tube. No bad result.

There were 119 excisions of the mamma in 110 patients. Among these were 75 cases in which the fat and glands in the axilla were also removed. Of the 110 cases 6 died; 1 from shock, 1 from anthrax, which Volkmann thinks must have been introduced with the cat-gut, 2 from exhaustion in old people, 2 from erysipelas—in 1 case arising from a bed-sore, and in the other commencing after the antiseptic dressings had been left off.

Up to this time Volkmann had treated seventy-three compound fractures and twenty-four wounds of joints conservatively without a single death.

Adding together the whole results, we find that 562 serious operations and injuries were treated, with twenty-nine deaths, *not one of these being due to infective disease arising under an*

antiseptic dressing. Volkmann further states that not a single case of pyæmia or septicæmia occurred among patients treated aseptically during these three years. Erysipelas attacked three or four of the cases which were treated on aseptic principles.

Here is a piece of evidence which cannot be overlooked. Into an infected hospital the aseptic method was introduced without any other change being made in the arrangements of the hospital. At once the infective diseases, which were attacking almost every patient, disappeared in the cases treated aseptically, only one case of pyæmia and about twelve of erysipelas occurring during more than four years, and almost all of these arising at the very commencement of the trial when as yet the surgeon had not had sufficient experience of the working of the method. When this experience was obtained, these diseases practically entirely disappeared. In estimating these results at their proper value, we must also remember that during the aseptic period, operations were performed and limbs preserved in a way impossible in an infected atmosphere. What would have been the result of the 150 operations on bones, or of the hydrocele cases, or of the wounds of joints, or of the compound fractures in the former infected atmosphere, if they had not been treated aseptically? What would their result have been in a good atmosphere, such as St. Bartholomew's hospital is said to possess? Would there have been *no* infective diseases there?

Similar remarkable facts have been published by Professor Nussbaum of Munich, who commenced aseptic treatment two years later than Professor Volkmann. The 'Allgemeines Krankenhaus' at Munich, though by no means very deficient in sanitary arrangements, became infected with septic diseases, so that almost every case of open wound treated in the wards was attacked by them. Pyæmia was rife, affecting nearly all cases of compound fracture or wounds of bones, amputation wounds, &c.; erysipelas was constantly present. During 1872, hospital gangrene also appeared, and steadily spread in spite of all the precautions which were taken. In 1872, twenty-six per cent. of all the wounds were attacked with hospital gangrene, and during 1873 the proportion increased to fifty and ultimately eighty per

cent. Erysipelas, too, which in 1872 was comparatively mild, became much more virulent and frequent. All this occurred in spite of the use of antiseptics, of the open method, &c.

In an address delivered in 1875, at the end of the academical year,[1] Professor Nussbaum mentions these facts and describes also the results which followed the introduction of strict aseptic treatment. With regard to his former results, to which I have just alluded, he says that he had employed in the treatment of wounds the open method, various forms of occlusion, continuous water-baths, chlorine water, carbolic acid, salicylic acid in powder and solution, Mr. Lister's carbolic paste, and even the carbolised gauze dressings. 'Alles, alles,' says he, 'war nicht im Stande, den Hospitalbrand, die Pyæmie zu bekämpfen.' Without any other change, strict aseptic treatment was used in all possible cases, and then, he says, at once they experienced one surprise after another: everything went well; there was no more hospital gangrene, though a week or two previously eighty per cent. of the wounds were suffering from it; pyæmia and erysipelas were only observed in one or two cases, and these disappeared as skill in the use of the method was acquired. Nussbaum adds: 'One might reply' (to these facts), 'pyæmia and hospital gangrene are diseases which often suddenly attack a hospital without any apparent cause and often also suddenly disappear. But think, my friends, that during the sixteen years in which I have had charge of this hospital pyæmia has not been absent a single month, and yet it suddenly *disappeared* on the introduction of the Listerian method.'

A year later, Dr. Lindpaintner, Professor Nussbaum's assistant, published a detailed account of Nussbaum's practice from April 1st, 1875, to the end of March 1876.[2] I shall not enter at length into these results, but there are some points to which I must call attention.

During this period there were 459 operation or accidental wounds under treatment, and of these twenty-six died. The cases were not all treated aseptically as will be evident when I consider the causes of death, to which I must now allude.

[1] *Die Chirurgische Klinik zu München im Jahre* 1875; Stuttgart: Fred. Enke, 1875.

[2] *Deutsche Zeitschrift für Chirurgie.*

Three cases in which primary amputation was performed died in a few hours. Other extensive injuries were present in all these instances.

One patient, æt. 79, died suddenly of cerebral hæmorrhage 31 days after resection of the elbow joint.

In one case a malignant tumour of the scalp was removed, and the disease was found to have perforated the skull. Recurrence rapidly took place, and the patient died with symptoms of compression.

One case of gunshot wound of the skull and brain went on well for 11 days, and then died suddenly. Cause unknown.

A large abscess of the mamma connected with caries of the ribs was opened aseptically, and was progressing typically, when death occurred suddenly on the 3rd day. Cause unknown.

One case of large ulcer of the leg, which had healed, died of phthisis nearly 4 months after admission.

One case of extensive abscess of the knee-joint, which was incised aseptically, died of phthisis 5 months after the operation, the knee-disease having recovered, and the wound having completely healed.

One case of extensive suppuration in the parotid region died. There was constant vomiting, and on post-mortem examination catarrhal pneumonia was found.

A very weak, unhealthy subject, suffering from compound fracture, for which secondary amputation was performed, died 5 days after the operation from thrombosis of the pulmonary artery.

One case of excision of the mamma (æt. 72) died, and on post-mortem examination there was found cancer of the lungs, capillary hæmorrhages in the stomach, and extensive aphthous patches in the œsophagus. Death was sudden after the existence of difficulty of breathing for a few hours.

In one case of excision of the mamma peritonitis set in on the 4th day, though up to that time the patient had been doing well. The peritonitis was found to proceed from a cancerous tumour in the liver, which was breaking down.

One case of excision of the mamma died on the 15th day from unilateral pleuro-pneumonia on the same side.

One case of large abscess in the neck died of 'fibrinous pericarditis' 38 days after the abscess was opened, and after it had healed.

One case of ovariotomy, in which there had formerly been peritonitis, and where extensive adhesions were present at the time of the operation, died of peritonitis.

One case of compound fracture of the leg, which was doing well, died of myocarditis on the 25th day.

One case of resection of the elbow-joint died of œdema of the lungs and fatty embolism on the 11th day.

One case of compound fracture of the femur in which the bone was very extensively comminuted and the knee-joint opened, died of septicæmia.

The remainder of the fatal cases occurred in patients not treated aseptically, and were due to septicæmia, erysipelas, pneumonia, pyæmia, phthisis, peritonitis, and shock.

I have thought it well to mention all the causes of death, and now I shall summarise what Dr. Lindpaintner says as to septic diseases.

Erysipelas occurred six times during that year, but not in any case which was being treated aseptically. It occurred in four cases of septic wounds, in one case after the Listerian dressings had been left off, and in one case of excision of the mamma treated aseptically but spreading from an inflammation in the neck and never extending under the dressing.

There was no case of hospital gangrene.

There were three cases of septicæmia—one after a septic operation (excision of the hip), one case admitted with septicæmia, and one case of compound fracture in which an unsuccessful attempt was made to eradicate the causes of putrefaction (alluded to above).

There were three cases of pyæmia—one occurring after a putrid wound in the thigh, one in a case with putrid sinuses near the elbow-joint, in which no operation was performed, and one after dilatation of a stricture of the urethra which had followed a previous amputation of the penis.

There was thus only *one case* of infective disease among the cases treated aseptically—the case of bad compound fracture of the femur. Here, of course, the surgeon had not to deal with a wound made by himself, but with one made without aseptic precautions, and it is of course always a matter of uncertainty in such instances whether the wound can be afterwards rendered aseptic or not. The case of peritonitis after ovariotomy and the cases of pericarditis and myocarditis might

no doubt be attributed to failure in the method. Whether or not they were due to such a cause, I do not know.

This success in the exclusion of traumatic infective disease has continued up to the present time in the cases treated aseptically, and in a publication of Professor Nussbaum's in 1878, entitled 'Sonst und Jetzt,' he states that there had been no further instance of these diseases. It may be interesting to see what he says:—

Formerly.	Now.
'Injuries of the head, compound fractures, amputations and excisions, in fact, almost all patients in whom bones were injured were attacked by pyæmia. For example, of 17 cases of amputation 11 died from this cause. Even patients with severe whitlow died of it.	No pyæmia.
'Hospital gangrene had got the upper hand to such an extent, that in spite of the open method, in spite of continuous water-baths, in spite of the use of chlorine water or the actual cautery, finally 80 per cent. of all wounds and ulcers were attacked, large arteries being opened into. . . .	No hospital gangrene.
'Almost every wound was attacked with erysipelas.	No erysipelas.'

Still later, in the last edition of his work on antiseptic surgery, published in 1880, the same statement is reiterated and Nussbaum now says, that since the introduction of the aseptic method there has been *no* instance of pyæmia, hospital gangrene or erysipelas among the patients treated in that way. And yet, he adds, no other change has been made; 'the wards, the furniture, the nursing of the patients and their number remain the same.'

Indeed, in summing up his five years' experience he goes so far as to say that 'any recent wound, treated by this method, is guaranteed against pyæmia, hospital gangrene, erysipelas, progressive suppuration, and in general against all accidental com-

plications.' And further: 'The fate of a patient seriously wounded is almost entirely in the hands of the surgeon who applies the first dressing.' Such is Nussbaum's experience after using this method for five years in a hospital in which infective diseases were very prevalent.

These facts cannot be overlooked, and are of the very greatest value—of much more value than any number of results in healthy hospitals. For here we have an immediate abolition of traumatic infective diseases, only one case occurring in five years, and that after a wound not made by the surgeon; there was thus not merely a great diminution in the frequency of these diseases but absolute cessation. These facts, when surgeons in this country have deigned to notice them, have been attributed to increased cleanliness alone, the result of the introduction of strict aseptic precautions. It is asserted, and the assertion no doubt holds good in many places, that dirty sponges, dirty instruments, &c., were used in dressing the various cases, and that no care was taken to cleanse the instruments after their use, nor to keep apart those employed in bad cases. I venture, however, to affirm that these objections do not apply to the practice of such men as Bardeleben, Esmarch, Hueter, Nussbaum, Volkmann, and many others whom I might mention—men who are at the head of the surgical profession, and who owe their high reputation to their thorough knowledge of physiology and pathology. In Professor Nussbaum's case this objection cannot be upheld for one moment, for he had charge of two hospitals, one in the country placed under good hygienic conditions, and the other in town not so well situated in these respects. The same surgeons and the same methods of dressing and nursing were employed in the one institution as in the other, and yet the country hospital remained healthy, while the one in town became infected. Surely the same uncleanliness would have told, at least to some extent, in the country as in the town. But further it must be remembered that the result of the use of the aseptic system was *not merely the diminution*, but the *abolition*, of these infective diseases. Now cleanliness, as advocated by Mr. Savory—and I shall refer to this matter again—does not result in the *abolition*, but merely in the *diminution* of these affections, and I do not allow that

the results which Mr. Savory gives were solely due to cleanliness as distinguished from asepticism.

Then, again, these diseases *disappeared at once*, for Nussbaum tells us that *from the day* when he began this treatment thoroughly, these diseases never attacked any of the cases so treated. What an extraordinary amount of cleanliness would be required to effect this! But indeed we are told that cleanliness alone had not been able to abolish these diseases, even after *several years' practice of it*, for Nussbaum says in 1878,[1] that even then a tracheotomy or other wound which could not be treated aseptically was liable to be attacked by erysipelas of as severe a type as formerly. And, further, Nussbaum had been practising cleanliness before the introduction of the aseptic method; for many of the wounds were irrigated with antiseptics, such as carbolic acid, &c., without any apparent benefit. Cleanliness is, no doubt, a most excellent thing in its own place, but its power as a preventive of infective disease in an infected atmosphere is very limited indeed.

And lastly, it cannot be said that Nussbaum does not now diagnose as infective disease cases which he would formerly have classified under that heading, for post-mortem examinations are made on all the fatal cases by Professor von Buhl, quite independently of Professor Nussbaum, and thus any such error would be corrected. Nussbaum remarks in a note to Mr. MacCormac ('Antiseptic Surgery'), 'the mortality is reduced to one half, and the only cases brought to the postmortem room are those of death by suicide, from severe mechanical injury, in old people, or from cancer and tubercle.'

Socin of Basle uses language very similar to that of Nussbaum with regard to the occurrence of infective diseases. He has also observed their complete disappearance under aseptic treatment, and as the result of his experience, he says that, 'Every case of amputation which dies of pyæmia or of erysipelas is a victim of ignorance, of want of skill or of negligence on the part of the surgeon.'

Saxtorph of Copenhagen has had an experience similar to that of Volkmann and Nussbaum. His results are quoted at considerable length in Lucas Championnière's 'Chirurgie Anti-

[1] *Sonst und Jetzt.*

septique,' to which I must refer the reader. I may, however, just mention the following facts.

Before 1873 Saxtorph had performed 15 excisions of joints, of which 9, or 60 per cent., died. These wounds were treated in the ordinary manner. He then introduced aseptic precautions, but they were very imperfectly carried out. During this period (between 1873 and 1877), he performed 76 excisions of joints with 32 deaths, or a mortality of 42 per cent. The method was then carried out more efficiently, and since 1877, 34 excisions of joints (including 15 of the hip and 12 of the knee), have been performed with 5 deaths, or a mortality of 17 per cent. Indeed, Professor Saxtorph says that since he has carried out the method with absolute strictness, according to Mr. Lister's directions, he has performed 24 major excisions with 1 death (from tetanus), or a mortality of 4·3 per cent. This result has also been obtained in a bad hospital where infective diseases were common. The increased success, according as more efficient precautions were taken to exclude the causes of putrefaction, is very interesting and important.

Another strong advocate of the aseptic method is Professor Esmarch of Kiel. In 1875, he introduced the method into his wards, which had previously been especially liable to attacks of erysipelas. The report of the first year in which Esmarch employed this method is published by Waitz in 'Langenbeck's Archiv' for 1877, and he states that 536 operation cases were under treatment during that year, and of those treated aseptically only four were attacked by erysipelas (one of these cases died). Two patients who were treated aseptically died without any definite symptom except the presence of a continuous high temperature—septicæmia (?). Two cases died of pyæmia, but neither of these tell against the method; one was a case of acute necrosis where an abscess was opened, but where, nevertheless, pyæmia carried off the patient; the other was a case of herniotomy, in which the gut was found to be gangrenous.

These results were very good for the first year of aseptic work, but as Esmarch and his assistants gained experience, the cases of infective disease became fewer and fewer. His report for 1878 is alluded to by Mr. MacCormac,[1] who states that

[1] *Antiseptic Surgery.*

Esmarch had during that year treated 524 cases with 25 deaths. These cases include forty amputations, 27 major resections, 80 cases of removal of tumours, &c. Mr. MacCormac gives a list of the causes of death, and among them were two from septicæmia after excision of the mamma, presumably performed aseptically, though that is not expressly stated; no facts are given with regard to them. One case of ovariotomy died from peritonitis. In this, as in many other reports, no separation is made between cases dressed aseptically and those treated by other methods.

Mr. MacCormac states that Professor Esmarch informs him that his next triennial report will show better results. A part of this report is already furnished by Dr. Neuber, in his last communication on absorbable drainage tubes and permanent dressings.[1]

Between the end of April and the beginning of October, 1879, all the cases treated aseptically were dressed in this way. These were 131 in number, comprising 5 amputations of the thigh, 7 amputations of the leg, 1 at the knee-joint, 1 through the humerus, and 1 through the fore-arm; 2 excisions of the knee-joint; 4 excisions of the mamma and axillary glands; 5 excisions of the mamma alone; 16 excisions of large lymphatic glandular tumours from the neck varying from 1 to 2 fists in size; 16 excisions of other tumours, &c. During this period 3 patients treated aseptically died, viz., one case of excision of the hip from shock; one case of removal of carcinoma from the ear of a patient (æt. 70), in which there were secondary affections of the glands in the neck, &c., from hypostatic pneumonia; and one patient, who was suffering from septicæmia before his thigh was amputated, and who afterwards died of that disease (not a case in point).

During the next 2 months 60 additional cases were treated in this way without a death. These comprised amputations, resections, compound fractures, removal of tumours, &c. Thus during these 8 months 191 cases were treated aseptically with 3 deaths, but in no instance did infective disease arise after the operation.[2]

[1] *Ueber die Veränderungen decalcinirter Knochenröhren in Weichtheils-wunden, &c.*; Langenbeck's *Archiv*, Bd. xxv.

[2] At the International Medical Congress of this year, Prof. Esmarch brought forward still more recent statistics. The cases were treated by Neuber's method of permanent dressings, the antiseptic chiefly employed in

Perhaps the earliest in Germany to take up asepticism was Professor Hueter of Greifswald, and he still looks on it as the best method and the 'greatest advance of modern surgery.' He uses salicylic jute instead of carbolised gauze, not because he believes it to be better, but because it is cheaper.

Professor Czerny of Freiburg also added his evidence in 1876.[1] The number of cases to which he alluded was not great, but in his address at the end of the summer session, 1876, he stated that he had been unable to show his class a single case of pyæmia, hospital gangrene, or septicæmia during the whole year. He had two cases of erysipelas, one of these occurring after sequestrotomy and attempted purification of the sinuses with chloride of zinc. (This was not a case operated on from the first aseptically.)

Czerny says that in former years, with the exception of 1875, during which aseptic treatment had also been employed, he had always had a considerable number of cases of infective disease in the wards. The abolition of these diseases could not, he says, be due to anything in the arrangement or service of the wards, for these remained unaltered. They were as full as formerly, as imperfectly ventilated, and the watercloset arrangements were unaltered. A greater number of patients were treated. The results had steadily improved with increased care in carrying out all the details; and his belief is that 'the favourable results which have followed the introduction of this method are to be ascribed to the accuracy with which Mr. Lister's directions have been followed.'

In France this method was first taken up by Dr. Lucas Championnière, whose text-book and other writings[2] on the

the dressings having been Iodoform. They had recently treated 398 cases of major operations and injury with 6 deaths. There was no instance of infective diseases. The cases were 146 excisions of large tumours, including 40 excisions of the mamma and axillary glands and 14 castrations with three deaths—1 from pericarditis and old syphilis, 1 from apoplexy, and 1 from fatty heart; 61 resections; 51 major amputations (18 of thigh, 27 of leg, 5 of arm, 1 of forearm), with 2 deaths—1 from shock and hæmorrhage, and 1 from delirium tremens; 11 exarticulations 26 necrotomies; 13 nerve stretchings, including one for tetanus, which was fatal; 8 herniotomies; 21 chronic abscesses; 12 large wounds; 49 compound fractures.

[1] *Berlin. Klin. Wochenschrift*, 1876, No. 43.

[2] *Chirurgie Antiseptique.*

subject, are now so well known. He also finds that pyæmia, septicæmia, and hospital gangrene disappear when aseptic treatment is employed. He has at times observed erysipelas under this dressing, but the disease was very rarely present and of a very mild type.

As the result of the writings of Lucas Championnière, several French surgeons have introduced this method, but it has not as yet taken the same hold in France that it has in Germany. Those, however, who have used it properly make the same statements as to complete disappearance of infective disease. Among those who have employed this treatment and who have got these results may be mentioned M. Gross of Nancy, who has written a text-book on the subject.

M. Létiévant, of Lyons, is also an enthusiastic supporter of this system.[1] He introduced it into his wards during the summer of 1875, and from that time pyæmia disappeared and the mortality after operations and wounds greatly diminished. His statements with regard to infective disease are: 'Purulent infection has disappeared. Hospital gangrene has disappeared. Erysipelas is much rarer and less severe.'

Professor Panas[2] also found that during a virulent epidemic, those of his patients at the Lariboisière who were treated aseptically escaped erysipelas and other septic diseases.

Returning to Germany, there still remain two pieces of statistics to which I must refer.

The first is Schede's comparative statement of the results of amputations treated aseptically and of those treated in other ways.[3] The aseptic cases were under the care of Busch, Schede, Socin and Volkmann. The cases not treated aseptically were furnished by Bruns, Bardeleben and Billroth.

The most important tables give the result of uncomplicated amputations performed aseptically, contrasted with those treated by other methods.

Uncomplicated amputations treated aseptically:—

[1] *Note sur le pansement antiseptique listérien à l'Hôtel-Dieu de Lyon*, 1880.

[2] *Gazette hebdomadaire*, 1878.

[3] *Amputationen und Resectionen. Handbuch der allgemeine und specielle Chirurgie*, Pitha und Billroth.

	Cases	Deaths
At shoulder-joint	9	1
Through humerus	32	—
„ fore-arm	47	—
At wrist-joint	4	—
„ hip-joint	6	4
Through thigh	86	6*
At knee-joint	3	—
Through leg	69	1†
Partial amputations of foot (including 9 Syme's amputations, 36 Pirogoff's, 15 Chopart's, 1 Lisfranc, 4 of metatarsal bones)	65	2§
Total	321	14

A mortality of 4·4 p. c.

* One of these deaths was due to septicæmia; † from 'habitual erysipelas;' § one from 'pyæmia simplex.'

Uncomplicated amputations treated by the older methods:—

	Cases	Deaths
At shoulder-joint	15	8*
Through humerus	41	6†
„ fore-arm	42	2
At wrist joint	10	—
„ hip-joint	3	2‡
Through thigh	105	43§
„ knee-joint	7	1‖
„ leg	115	38¶
Partial of foot (8 Syme's amputations, 13 Pirogoff's, 5 Malgaigne's, 9 Chopart's, 1 Lisfranc, 3 of metatarsal bones	39	10**
Total	377	110

A mortality of 29·18 p. c.

*† All from infective disease; ‡ one from pyæmia; § thirty-eight from infective disease; ‖ from pyæmia; ¶ thirty-seven from infective disease; ** nine from infective disease.

Other tables are given stating the results of double amputations, of amputations with other severe injuries, of amputations in existing septicæmia, pyæmia or tetanus, and of deaths from intercurrent diseases quite unconnected with the operation. I need not, however, go into these, as they would only obscure the point at issue.

Schede tabulates the causes of death in the two preceding tables as follows:—

	In the septic cases	In the aseptic cases
Pyæmia	72	0
Septicæmia	19	1
Erysipelas	2	1
Trismus	0	1
Pyæmia simplex	6	1
Hæmorrhage	3	1
Exhaustion	2	1
Shock	6	8
Total	110	14

Thus, as Schede truly remarks, if the deaths from infective diseases were removed from the list of septic cases, the death-rate in each would be almost the same—about 5 per cent. for the septic cases, as against 4·4 per cent. for the aseptic. In other words, the aseptic method saved this enormous proportion of lives in the main by preventing infective disease.

The last paper to which I shall refer is Reyher's account of his results during the Russo-Turkish war.[1] As I intend to discuss these results later I shall just mention the numbers here.

Eighty-one cases were treated aseptically as soon as possible after the injury, and of these 15 or 18·6 per cent. died. Among these cases were 27 gunshot wounds of joints treated conservatively throughout with 4 deaths; 19 primary resections in cases of gunshot wounds of joints with 2 deaths; 13 primary amputations with 5 deaths; and 22 compound fractures with 4 deaths. Of the 15 deaths, 5 were due to septic diseases (2 to pyæmia, and 3 to septic inflammation and suppuration). With regard to the deaths from septic diseases in these cases, we must remember that here the surgeon had not to deal with an operation performed by himself, in which the problem is to *keep out* the causes of putrefaction, for here the causes were in many instances already present. The deaths occurred in cases where these causes were present and were not destroyed because portions of clothing, &c., were present in recesses of the wounds.

Contrasted with this were 65 compound fractures and 78

[1] Volkmann's *Sammlung*, Nos. 142 and 143; *Die antiseptische Wundbehandlung in der Kriegschirurgie*, 1878.

wounds of joints, in all 143 cases, which did not come under treatment for some time, and in which the generally unsuccessful attempt at purification was made. Where these attempts are unsuccessful the treatment resolves itself into treatment with antiseptics. Of these 143 cases treated with antiseptics, 71 or 49·6 per cent. had already died when the first part of the report was issued, and 2 more died subsequently. Of these at least 46 cases died of septie diseases.

Then contrasted with both these sets of cases there were treated alongside of them 62 wounds of joints, in which no attempt was made to render the wounds aseptic, or even to treat them with antiseptics. Of these, 48 or 77·4 per cent. had died when Reyher wrote, and amputation or resection had been found necessary in most instances.

Lastly, I would allude to the results obtained in ovariotomy by the leading ovariotomists of this country. Mr. Spencer Wells and Dr. Keith had for years striven to carry out perfect cleanliness and other antiseptic preeautions short of complete exclusion of organisms from wounds, and the results of both were very remarkable indeed. And yet, since they have extended their precautions so as to *exclude* the causes of fermentation, their results have still further improved, and they themselves—and they are the best judges—attribute this last improvement solely to the additional antiseptic precautions employed.

Mr. Knowsley Thornton, in his speech at the debate on antiseptic surgery in December, 1879,[1] gave an analysis of more than 300 cases of fatal ovariotomy, and stated that in more than one-third of the cases septicæmia, pyæmia or septic peritonitis were given as the cause of death, and in nearly another one-third the fatal result was attributed to peritonitis. Mr. Thornton pointed out further that the cases of peritonitis were almost entirely due to septic causes, and thus, as he states, two-thirds of the deaths after ovariotomy were due to septie influences, and therefore, if these influences can be eliminated by aseptic surgery, the results ought to improve to a corresponding extent. And he states that this has been his experience, and that the introduction of strict aseptic precau-

[1] MacCormac's *Antiseptic Surgery*.

tions has reduced his mortality by half, and it would have been still less had not putrefaction been present in some of the cases before he operated, owing to previous tapping of cysts by other surgeons without aseptic precautions.

Mr. Spencer Wells, at the same meeting, spoke very strongly in favour of aseptic treatment, and gave the result of the last 168 cases which he had treated in private practice. The first 84 had been treated by his former methods, but yet, he says, 'with all the care I could give to them there were 21 deaths.' On the other hand, the last 84 were treated aseptically, and of these only 6 died, and these deaths occurred among the earlier cases while he was only as yet making acquaintance with the details of the method; and so 'as I went on and became still more accustomed to the method and details of antiseptic treatment, and avoided mistakes, then I obtained the long run of 38 cases without a single death; and, adding to that the 5 more of other important abdominal operations, I can record the gratifying and almost incredible result of 43 cases of these great operations without a death.' In a foot-note he adds: 'The run of 38 cases of successful ovariotomy was afterwards increased to 41, and then a death occurred where septic symptoms had set in before operation.'

Dr. Thomas Keith, who had adopted the aseptic method somewhat earlier, and who had previously been getting the best results of the day, states[1] that his last 76 cases were performed aseptically with only 2 deaths, and these occurred at the commencement, so that the last 68 cases in succession all recovered. Dr. Keith is equally positive in ascribing these good results to the additional precautions which he had taken.[2]

[1] See MacCormac's *Antiseptic Surgery*.

[2] At the recent International Congress held in London, Dr. Keith is said to have stated that he had discontinued the use of the spray. I have not been able to find a report of his statement, but I can quite imagine that the amount of carbolic acid poured into the peritoneum by the spray in a prolonged operation would be injurious both from rendering the patient liable to carbolic acid poisoning, and also from causing an increased amount of fluid in the peritoneal cavity, which, where the details of the aseptic method were not thoroughly carried out, would be liable to undergo putrefaction. Though Dr. Keith has given up the spray, I have not heard that he has given up the practice of aseptic surgery. (See Chapter XVIII. for remarks on ovariotomy.)

Salicylic acid, which was introduced by Professor Thiersch [1] as a substitute for carbolic acid, has been employed in various hospitals, but the results do not seem to be so good as those with carbolic acid. The following is a specimen of Thiersch's earlier results:—

From April 1st, 1874, to May 31st, 1875, 109 cases were treated with salicylic acid dressings, and 51 with carbolic acid. Of the former 7 died, of the latter 2. Among the 109 salicylic acid cases were 21 major amputations, with 5 deaths; also a number of resections, compound fractures, abscesses, excisions of tumours, &c. The deaths among the cases treated with salicylic acid were due to the following causes: In one case from hæmorrhage from the intestine 100 days after primary amputation of one leg and secondary amputation through the other femur; in one case from hydrothorax 123 days after primary amputation of the leg; one patient died on the twenty-third day after amputation of the thigh, pus being found in the shoulder-joint; a case of amputation of the thigh died from exhaustion twenty-eight days later; one case of excision of the head of the femur died from uræmia twenty-five days after the operation; one resection of the wrist-joint, followed by amputation of the fore-arm, died 201 days after the first operation from exhaustion. One of the deaths in the cases treated by carbolic acid was from pyæmia.

Erysipelas attacked the salicylic acid cases seven times, and in one instance proved fatal.

The general description of the course of these cases is not so good as that of cases treated with carbolic acid, and Volkmann and others have tried salicylic acid, but have not found it so trustworthy as carbolic acid.

Thymol, though at first much praised,[2] also soon failed to give satisfaction. The explanation of this probably was that in the first instance the thymol was used in wards free from infective diseases, and therefore good results were at first obtained. But these wards had a great tendency to become unhealthy on account of the bad hygienic conditions, and the

[1] 'Klinische Ergebnisse der Lister'schen Wundbehandlung und über den Ersatz der Carbolsäure durch Salicylsäure'—*Volkmann's Sammlung*, Nos. 84 and 85, 1875.

[2] *Ueber das Thymol und seine Benutzung bei der antiseptischen Behandlung der Wunden*, von Hans Ranke, *Volkmann's Sammlung*, No. 128.

thymol was unable to prevent this in the same way as carbolic acid had done. Thus bad results were very soon obtained, and carbolic acid had to be reinstated.

Here is another strong argument against the idea that cleanliness alone is a sufficient safeguard. In this instance there was of course the same amount of cleanliness when thymol was used as when carbolic acid was employed, but a powerful antiseptic was required in addition to the cleanliness, and, as the experience before the introduction of Listerism into these wards had shown, it was necessary to use this antiseptic on aseptic principles in order to attain the desired object.

The Relation of other Forms of Antiseptic Surgery to Infective Diseases.

I have found the greatest difficulty in getting records of any value as to the results of treatment with antiseptics not employed aseptically. I have described Bilguer's method and results (see p. 302), and these may be taken as a very fair specimen of the results obtainable by treatment with antiseptics alone, though it must be observed that in many instances his cases were no doubt treated aseptically. I have also, at p. 401, referred to Reyher's paper giving details of the different results obtained by aseptic treatment and treatment with antiseptics, and this is perhaps one of the best contrasts which could be given. Several of the results which have been published as having been obtained by strict aseptic treatment are in reality nothing of the kind, but are merely the results of treatment by antiseptics and they might very fairly be used as such. And it would be seen from these that though infective diseases are often much diminished in frequency, yet they are by no means entirely abolished. Indeed, the rapid manner in which carbolic acid began to fall out of use before Mr. Lister published, shows that it was found to be inefficient when used as an antiseptic only and not aseptically. I believe that, employed as an antiseptic only, carbolic acid is inferior to various other substances, for it combines with albumen, and in doing so apparently loses in part its antiseptic virtues (see p. 260); and therefore, in order to be efficient, it would require to be added to the discharges of the wound in large quantities. At the same time its in-

efficiency is increased, because carbolic acid is very irritating, and causes an increased amount of discharge liable to undergo fermentation. And also, as Hack[1] has shown, wounds treated with carbolic acid have greater absorbing power than those treated with other substances.

The best comparison between aseptic treatment and treatment with antiseptics is that furnished by Mr. Lister's own results (see p. 376). The cases which were not treated aseptically were, as far as possible, treated with antiseptics. They were frequently syringed with antiseptic lotions and dressed with antiseptic ointments and dressings, and yet it will be seen, that the proportion of deaths avoidable by methods of wound treatment, and especially of deaths from blood-poisoning, was much greater than in the aseptic cases, although the nature of the injuries was much less severe.

At p. 348 I have referred to the results obtained by the use of alcohol in Nélaton's practice, and I stated that in Chedevergne's paper[2] mention is made of 48 cases treated in this way, of which only 1 died of pyæmia.

The cases were, however, not very severe. Thus, there was 1 case of tumour of the lower jaw; 3 cases of epithelioma of the lip; 10 partial excisions of the mamma; 2 amputations of the leg; 5 cases of removal of fatty tumours, and a number of isolated minor operations. There were also 2 cases of wound of the knee-joint and 1 of the elbow-joint, all of which recovered. No details of these cases are given.

Rochard[3] extends these statistics, and states that 97 patients had been treated in this way, and that among these there had only been 2 cases of pyæmia and 5 of erysipelas.

Chedevergne states with reference to the first part of these statistics that the results obtained were not merely accidental, for two cases which had not been treated with alcohol died of pyæmia. He attributes the fatal case from pyæmia, mentioned in his paper, to imperfect application of the dressing, pus having been allowed to accumulate in the wound.

[1] *Ueber das Resorptionsvermögen granulirender Flächen*, Leipzig, 1879.

[2] 'Du traitement des plaies chirurgicales et traumatiques par les pansements à l'alcohol,' *Bulletin général de thérapeutique*, vol. 67, 1864.

[3] *Histoire de la Chirurgie Française au XIXe Siècle*, Paris, 1875.

In London most excellent results have been obtained by Mr. Hutchinson by the use of a spirit and lead lotion (see p. 269); and in his speech at St. Thomas's Hospital, at the debate on antiseptic surgery,[1] he referred to his results, and stated that they were as good as those obtained by a colleague who practised aseptic surgery, but he added that he himself had abstained from operations which involved peculiar risk.

There can be no doubt of the excellence of alcohol as a dressing, and the good results are to some extent explained by Hack's[2] experiments, which show that absorption takes place with the greatest difficulty from wounds treated with alcohol. No doubt, also, as Mr. Hutchinson uses it, many of the cases are treated aseptically.

With regard to these and other results from the use of antiseptics, I cannot give any tables; for few surgeons have employed one antiseptic or one particular method of applying them, and in London more especially, where almost every surgeon uses one or other form of antiseptic treatment, the results from the various methods of treatment are grouped together, and are thus almost absolutely useless for the point at issue.

Among the best of these mixed results—results obtained no doubt in the main by the use of antiseptics—are the cases of major amputations performed at St. Bartholomew's Hospital for the last ten years (1870–79).[3] There were 467 major amputations, of which 71, or 15·2 per cent., died. These results include, however, the practice of two surgeons who treated their cases in the main aseptically.

We have already seen in the historical part that the various methods of occlusion, acting on the principle of excluding the gases of the air, have failed to exclude infective diseases. I need only refer to Jules Guérin's experience during the siege of Paris, p. 325, and to his refusal to apply his method in a particular hospital because the atmosphere was '*aussi profondément infecté*.'

The only method of occlusion which has been of permanent

[1] See MacCormac's *Antiseptic Surgery*. [2] Loc. cit.
[3] *St. Bartholomew's Hospital Reports*, 1880.

service is Alphonse Guérin's 'Pansement ouaté' (see pp. 280 and 325). As we have seen, marked improvement followed its introduction during the siege of Paris, and good results are still obtained in some of the less healthy Paris hospitals. This method no doubt acts mainly by keeping the layer of granulations at perfect rest, and thus avoiding its laceration and the consequent passage of the putrid material—that 'terrible poison,' as Mr. Savory has called it—into the blood. However good its results may have been in some cases, I cannot think that a method of treatment in which fluids, undergoing decomposition to a greater or less extent, are retained in contact with the surface of the wound, and in which the patient is only protected from the effects of the absorption of these fluids by maintaining the parts at perfect rest, is one which can be recommended when better means are obtainable.

The method which perhaps stands next to the aseptic method in its power of preventing infective disease is treatment by irrigation or the water-bath. Here also definite statistics fail, but I may refer to Langenbeck's statement (see p. 344), made in 1855, to the effect that during the preceding five or six years no case of pyæmia had occurred among the cases treated by the continuous water-bath. No results are given as to the other infective diseases, but from what I know and have seen of this method, I should think that where the water is frequently changed, especially if an antiseptic is added to it, and where the wound is not complicated, and there is no retention of discharges, these diseases would be more or less completely absent. Langenbeck mentions that during the five years to which I have just referred, in which he had no case of pyæmia among the cases treated by the water-bath, pyæmia was, nevertheless, prevalent in other wards, and attacked cases treated in other ways in the same wards.

I referred, also, on p. 345, to Valette's success. He employed antiseptics to a much greater extent than Langenbeck.

I now come to the consideration of the results obtained by the use of the open method.

I have already mentioned (p. 332) Bartscher and Vezin's results. They had 28 cases of amputation (26 of these being

major amputations), with 3 deaths. The causes of death are not given.

I have also mentioned (p. 333), Burow's results up till 1866. He had 94 amputations (87 of these being major operations), with 5 deaths. The causes of death are not stated. In a later paper (1877), Burow (junior),[1] gives the results of all the amputations performed up to that time by his father and himself. Since 1866, 29 major amputations had been performed with 4 deaths, thus giving a total of 123 amputations with 9 deaths, or more properly 116 major amputations with 9 deaths, or a mortality of 7·7 per cent. Of the 4 last deaths 2 were due to gangrene of the stump and 2 to pyæmia. How many, if any, of the other deaths were due to septic causes we do not know.

The following is the detailed list :—

	Cases	Deaths
Amputations through thigh	33	6*
,, ,, leg	25	3
,, ,, humerus . . .	25	0
,, ,, fore-arm . . .	29	0
,, of foot	9	0
Partial of hand	2	0
Total . . .	123	9

* 2 certainly from pyæmia.

These results are certainly remarkably good. The cases extended over a period of something like forty years, and were treated in a small hospital composed of four rooms containing altogether sixteen beds. These rooms were small and the quantity of air for each patient was not very great. A number of operations other than amputations were also performed. Thus, during the last ten years given in the paper to which I refer (1866–76), there were treated, besides the 26 amputations, 53 excisions of the mamma, 30 cases of fistula in ano, 4 amputations of the penis, 14 excisions of large tumours, 5 excisions of the upper jaw, 8 excisions of the lower jaw, 6 herniotomies, 6 tracheotomies, 5 lithotomies, &c. Thus the conditions were not so favourable as might at first sight be supposed. We are not told what the results were in these latter cases, either as to the occurrence of infective disease or otherwise. Burow

[1] *Archiv für klinische Chirurgie.*

took the most scrupulous precautions as to cleanliness, more especially of hands and instruments; for example, he never employed sponges which had been used before. Then in many of his cases, notably in the excisions of the mamma, he used acetate of alumina—a very powerful antiseptic—and he specially praises its property of keeping down smell.

Much more important facts are published by Krönlein [1] in his report of the results of the open treatment at Zürich from 1867–71. The method employed was chiefly that of Bartscher and Vezin, but it was combined with frequent irrigation of the wound with antiseptic solutions, so that we have here, not the open method pure and simple, but a combination of it with irrigation and treatment by antiseptics.

Krönlein gives details of the cases of amputation, excision of the mamma and compound fracture conservatively treated, and contrasts the results obtained between 1867 and 1871 with those of the previous 7 years (1860–67). During these 7 years all sorts of methods of treatment were employed. In some cases the edges of the wound were brought together by strips of plaster or by stitches, and covered with compresses and bandages; in others the wound was left open for several hours, &c.

Krönlein discusses from various points of view the causes of the difference in the results in the two periods, and at length comes to the conclusion that it is in the main owing to the different methods of treatment employed. I need not enter into all his arguments: I quite agree with his conclusion.

During the first period (1860–67), 260 important cases were treated, and of these 105 died. These cases included 140 amputations, 34 excisions of the mamma, and 86 compound fractures treated conservatively. Of the 105 deaths 59 occurred from pyæmia and septicæmia alone.

During the second period (1867–71), 172 similar cases were treated with 34 deaths. These consisted of 85 amputations, 22 excisions of the mamma, and 65 compound fractures. Of the 34 deaths 12 were from pyæmia and septicæmia alone. I may just quote his tables:—

[1] *Die offene Wundbehandlung.* Zürich, 1872.

Amputations.

1860–67.

	Cases	Deaths	From pyæmia and septicæmia
Thigh	36	31	16
Leg	36	21	12
Humerus	18	10	8
Fore-arm	24	4	2
Hand	9	—	—
Foot	17	6	4
Total	140	72*	42†

* Or a mortality of 51·4 p. c. † Or 30 p. c.

1867–71.

	Cases	Deaths	From pyæmia and septicæmia
Thigh	28	10	2
Leg	11	2	1
Humerus	14	2	2
Fore-arm	10	—	—
Hand	7	—	—
Foot	15	3	1
Total	85	17*	6†

* Or a mortality of 20 p. c. † Or 7 p. c.

Excisions of the Mamma.

1860–67.

Cases	Deaths	From pyæmia and septicæmia
34	11*	4†

* Or a mortality of 32·3 p. c. † 11·7 p. c.

1867–71.

Cases	Deaths	From pyæmia and septicæmia
22	3*	1†

* Or a mortality of 13·6 p. c. † 4·5 p. c.

Compound Fractures treated conservatively to the end.

1860–67.

	Cases	Deaths	From pyæmia and septicæmia
Thigh	7	2	0
Leg	62	13	7
Humerus	7	3	3
Fore-arm	10	4	3
Total	86	22*	13†

* Or a mortality of 25·5 p. c. † 15·1 p. c.

1867–71.

	Cases	Deaths	From pyæmia and septicæmia
Thigh	11	2	0
Leg	31	9	5
Humerus	13	2	0
Fore-arm	10	1	0
Total .	65	14*	5†

* Or a mortality of 21·5 p. c. † 7·6 p. c.

Krönlein does not give full details of the causes of death during either period, so that one cannot judge for one's self how far they were avoidable or not. He confines his attention to infective diseases.

If we analyse Krönlein's tables of amputations in the same way as was done on p. 387, for the septic and aseptic tables, i.e., if we only consider uncomplicated amputations, leaving out of consideration double amputations, amputations where other injuries were present, amputations in patients already suffering from septic poisoning, &c., we get the following results in cases treated by the somewhat modified open method described above :—

Uncomplicated Amputations.

(1867–71.)

	Cases	Deaths
Humerus	13	3
Fore-arm	8	0
Hand	7	0
Thigh	22	5
Leg	5	0
Partial of foot	12	3
Total . .	67	11

or a mortality of 16·4 p. c. as compared with Schede's result in aseptic cases of 4·4 p. c. Of these deaths one occurred from shock ; in five no cause of death is given ; four died from pyæmia ; and one from erysipelas.

Between 1860 and 1867 about 4,000 patients, suffering from all sorts of affections, were treated in the surgical wards at Zürich, and of these 146 died of pyæmia and septicæmia.

Between 1867 and 1871 about 2,300 similar cases were treated. Of these only 19 died of pyæmia and septicæmia.

Among the 4,000 patients of the first period erysipelas oc-

curred 148 times. Among the 2,300 patients of the second period erysipelas occurred 127 times.

We thus see, that a treatment consisting of a combination of the open method with intermittent irrigation and treatment with antiseptics reduced in a very marked degree the mortality from pyæmia and septicæmia, but did not affect erysipelas at all. What the open method alone would have done we do not know, but these results are very good in a hospital where infective diseases were prevalent. Krönlein himself says that these cases 'sufficiently show that the open method is no absolute guarantee against pyæmia and septicæmia.' He also states that 'the open method does nothing against erysipelas; indeed, during the time in which the open method was employed, erysipelas was more frequently observed than formerly.'

We have also already seen from the experiences of Nussbaum and Volkmann, that the open method is not very powerful against septic diseases in infected hospitals. Thus, for example, Volkmann states that at first he was an advocate of the open method, but that by-and-by, as the hospital became more and more unhealthy, the treatment became of less and less value.

I may, in contrast to the results of the foregoing methods, give a few statistical tables of cases not treated aseptically at all.

Billroth's results just quoted in Krönlein's book represent the mortality after operations not treated aseptically.

Malgaigne's statistics[1] are well known. The following table of amputations represents the results obtained in Paris at the time when he wrote.

	Cases	Deaths	
Amputation of thigh .	201	126 =	mortality of 62·6 p. c.
" " leg . .	192	106 =	" " 55·2 "
Partial of foot . . .	38	9 =	" " 23·6 "
Shoulder-joint . . .	13	10 =	" " 76·9 "
Humerus	91	41 =	" " 45 "
Fore-arm	28	8 =	" " 28·5 "
Total .	563	300 =	" " 53·2 "

[1] *Archives générales de Médecine*, 1842.

Paul[1] has collected a very extensive series of statistics, of which the following is a sample.

		Cases	Deaths		
Amputation at hip-joint	.	222	159 =	mortality of	71·6 p. c.
" through femur	.	1721	863 =	" "	50·1 "
" at shoulder-joint		192	84 =	" "	43·7 "
" " knee-joint		49	24 =	" "	40·9 "
" through leg	.	1242	480 =	" "	38·7 "
" " humerus		943	314 =	" "	33·3 "
" " fore-arm		391	73 =	" "	18·7
Total	.	5,060	1,997 =		39·4

Mr. Holmes in 1866[2] gave statistics of the last 300 amputations performed at St. George's Hospital. Of these, eighty-three or 27·6 p. c. died. In 1874[3] he published statistics of the last 500 cases of amputation, and, of these, 158 or 31·6 p. c. died. It thus appears that of the last 200 amputations performed at St. George's Hospital between 1866 and 1874, 75 or 37·6 p. c., died. This result shows that but little benefit had been derived from the recent improvements in surgery as regards cleanliness, &c., apart from the use of strict aseptic treatment, because the majority of these cases were treated after attention had been called to the subject by Mr. Lister's writings.

Mr. Erichsen[4] says that the mortality after great amputations varies from 35 to 50 p. c.

The results obtained at St. Bartholomew's Hospital were brought forward by Mr. Savory at the meeting of the British Medical Association at Cork[5] in 1879 to show what could be done simply by cleanliness and good air independently of aseptic treatment. The report published in 1880[6] shows that of 619 operation cases of all kinds (excluding eye operations) 45 or 7·2 p.c. died. Many of these operations were of very minor importance indeed. They included, however, 73 major amputations with 11 deaths; 13 excisions of joints with 4

[1] *Die conservative Chirurgie der Glieder*, Breslau, 1854.
[2] *St. George's Hospital Reports*, vol. i. [3] *Ibid.* vol. viii.
[4] 'On Hospitalism and the Causes of Death after Operations,' 1874.
[5] *British Medical Journal*, August, 1879.
[6] *St. Bartholomew's Hospital Reports.*

deaths; 29 cases of removal of tumours of the mamma, no death; 13 cases of removal of tumours of the tongue, 1 death; 4 lithotomies, no death; 32 herniotomies, 6 deaths; 25 tenotomies, 1 death; 74 operations for phimosis; 36 cases of fistula in ano, &c.

Among the 45 deaths were 9 from pyæmia and four from erysipelas. The causes of death in the other cases are not given. Erysipelas attacked 17 cases operated on, and 11 others. One very striking case is mentioned. The femoral artery was ligatured with various aseptic precautions for popliteal aneurism. The wound did well till the twelfth day, when hæmorrhage occurred. It was then re-opened without aseptic precautions, and the patient died of pyæmia.

Thus, by the use of cleanliness alone, infective diseases are by no means banished from St. Bartholomew's Hospital. Indeed, in considering these results, it must be remembered that a considerable number of the cases were operated on aseptically. How many cases were treated by good surgery and cleanliness alone, without aseptic precautions, and with what results, we do not know.

Very good results obtained by methods which are not very powerfully antiseptic were published in the spring of 1880 by Dr. McVail.[1] During the preceding three years 107 operations had been performed of which 50 were major operations. Of these 50 cases, 3 died. Among them were 31 major amputations, and the three deaths occurred in these cases; in one it was due to internal injuries; in one it followed gangrene of the back, due to contusion; and in one pyæmia was the cause. The onset of the pyæmia in the last case is said to have followed the opening of an abscess over the sacrum, and death followed within forty-eight hours—a very rapid course for pyæmia! Was it simply a case of sapræmia, or was the abscess part of the pyæmia? Then there were 3 cases in which joints were opened, viz., one compound dislocation of the elbow, which was reduced, but the local result is not given; one case of excision of the head of the radius, result not mentioned; and one case of excision of a metacarpal bone in which some joint was opened.

[1] *British Medical Journal.*

Of course these are a very small number of operations in three years, and they were treated in a country hospital, so that the result is not at all surprising; but when Dr. McVail attempts to draw extensive conclusions from them, and to show that the method of treatment adopted in these cases is better than the aseptic method, he is using a fallacious argument. All that can be said is that under the conditions in which these operations were performed, aseptic treatment was but little necessary, though even here we find that one out of 31 major amputations died of pyæmia. As we have seen, the best surgeons, Mr. Spence or Mr. Savory for instance, even with the use of the most scrupulous cleanliness, cannot reckon on anything like absence of infective diseases.

After all, these statistics are not nearly so good as the results obtained by Bardenheuer with aseptic treatment in the Cologne Infirmary in one year.[1] He had *no death* among 133 aseptic operations involving bones. These included, according to MacCormac—

41 amputations (17 of thigh) through bones.
10 ,, at joints (1 at hip).
53 resections (15 of hip and 12 of knee).
23 cases of removal of wedge-shaped pieces of bone.
5 operations for badly united fracture.
1 case of trephining.

[1] MacCormac's *Antiseptic Surgery*, p. 29.

CHAPTER XVIII.

RESULTS OF ANTISEPTIC SURGERY (*continued*).

General considerations. Wounds of, and operations on, healthy joints. Method of treatment adopted in these cases. Definition of the term 'Aseptic course:' Example. Wounds of healthy joints. Operations on healthy joints. Objections to the value of these cases: reply. Incisions into joints affected with synovitis. Incisions into joints affected with pulpy degeneration of the synovial membrane—*a* without suppuration, *b* with suppuration. Volkmann's results: Max Schede: Paul Barth: Saxtorph: Piéchaud: Nussbaum: Albert: Hueter: Létiévant: Kraske: Reyher: Bergmann. Comparison of Reyher's results with those obtained during the Crimean war, and with Heintzel's. Treatment by irrigation. Necessity for observing the minutest precautions as shown by Mr. Lister's case. Results of removing foreign bodies from joints without aseptic precautions: Larrey: Spence: Paget.

So far I have been dealing with general statements; and though these are often not of much use, yet I cannot think that there can be any doubt as to the great value of the facts which I have narrated. Thus, for example, we have had an opportunity of comparing Mr. Lister's results in Glasgow before and after the introduction of aseptic treatment. After he went to Edinburgh, we were able to trace a very marked improvement following the introduction of the aseptic method; and we also had the opportunity of contrasting the results of aseptic treatment with those of treatment by antiseptics in Mr. Lister's own hands. We were further able to point out that there was a great difference between these results and those obtained during the greater part of the same period, in the same hospital, and under more favourable hygienic conditions, by a surgeon who did not practise aseptic treatment.

We have further had most striking evidence from abroad showing that in infected hospitals the aseptic method has done what other methods, such as the open method, treatment by

irrigation or by antiseptics, could not do. It has not only diminished, but when properly employed, it has abolished infective disease. That this result has not been simply due to cleanliness, as has been suggested by some, is shown by numerous facts, such as those mentioned by Nussbaum (p. 394) and Volkmann, and also by the results of the use of thymol (p. 404).

Nor must I forget to mention the results obtained in ovariotomy, although these are not test cases. For the peritoneum has a wonderful power of destroying causes of putrefaction, or of rendering them inert by rapidly removing the fluid in which they might grow. Thus, dust-laden air has been injected into the healthy peritoneal cavity without bad results, though, if ascitic fluid were present, or if the peritoneum were unhealthy, there would almost certainly be fermentation, and probably, as a result of this, depending on the amount and nature of the irritating products formed, peritonitis. In the healthy peritoneum, which absorbs fluids with immense rapidity, there is no fluid for the organisms to develop in, and thus they are left in contact with active healthy living tissue, which, as we have seen from experiment, rapidly destroys them. Hence the case of ovariotomy is by no means a test.

There are certain cases, however, to which I must now allude, in which there is not the same tendency to destruction of organisms, but where there is opportunity for them to develop, and where the admission of septic dust is liable, as experience has shown, to be followed by very serious consequences. An example of such a case is where organisms are admitted into a healthy joint. Here fluid is present, in which they can develop, and here also all parts of the living surfaces are not in contact, and, therefore, organisms may be present in the fluid of the joint and yet not in contact with healthy living tissues. This is, then, a test case, for here organisms, if admitted, will as a rule grow. We must therefore inquire, how injuries which might involve the entrance of organisms into healthy joints behave under the various methods of treatment.

Then, again, in chronic abscesses we have similar conditions. Here the fluid is practically under the same conditions as if it were in a flask; the walls of the cavity are probably not even

healthy. How do such abscesses behave under various circumstances?

And lastly, although this is not such a test case, we know that compound fractures, whether made by the surgeon or by accident, are very dangerous, chiefly on account of their great liability to be followed by infective disease. In this instance the destroying action of the blood clot and of the living tissues can come into play, but nevertheless the movements to which the part is liable are apt to interfere with their action. Blood clot is only of use as a destroyer of bacteria if it be kept at rest.

Wounds of, and Incisions into, Healthy Joints.

I have already published the cases which have occurred in Mr. Lister's practice from the end of 1871 up to November 1879,[1] but I shall introduce these tables here in order to complete the subject. Before, however, discussing the results as a whole, I must mention how the cases have been treated, and what is the usual aseptic course.

Wounds of joints are treated on the principles described at page 113. If the case is seen very shortly after the accident (within a few hours) the joint is thoroughly washed out with 1–20 carbolic lotion by means of a syringe and catheter, the wound being enlarged if necessary; the surrounding skin is well washed with the same lotion, a drainage tube is introduced into the joint, a large gauze dressing is applied, and a splint is arranged so as to prevent movement. This treatment is carried out under a spray of carbolic acid. If the wound does not come under observation till after the lapse of twenty-four or thirty-six hours, a solution of carbolic acid in methylated spirit (1 part of carbolic acid to 5 of spirit) is used. If a still longer time has passed since the receipt of the injury, fermentation of the fluids in the joint has generally taken place, and there is but little hope of eradicating it. An attempt may, however, be made by the use of either of the lotions just mentioned, or of a solution of chloride of zinc (40 grs. to the ounce) or of iodoform suspended in alcohol and water.

In operating on healthy joints, the various precautions

[1] *British Medical Journal*, November, 1879

described in the chapters on aseptic surgery must be carefully carried out. As a rule the 1–20 carbolic lotion is used for all purposes. When the operation is concluded, a drainage tube or a horse-hair drain is introduced into the joint so as to provide free escape for the serum, which will probably, for a few hours, be of considerable amount, owing to the irritation of the synovial membrane by the manipulations and by the carbolic acid. The drain can generally be dispensed with in simple cases, such as after the removal of a loose cartilage, in from one to three days. As the result of these operations, the discharge becomes very slight after the first twenty-four hours. There is no pain nor swelling, in fact no local inflammatory disturbance whatever, and therefore, of course, no suppuration. Constitutionally the patient remains quite unaffected: he feels well, eats well, sleeps well, and in fact, thinks it a great hardship to be kept in bed for a few days. In the following cases, when I use the term 'aseptic course,' I mean this condition of absence of local or constitutional disturbance.

Let me take as an example a case of recent fracture of the patella, which was operated on with the view of obtaining bony union.

W. T. æt. 37 was admitted to King's College Hospital on Dec. 13, 1879, suffering from a recent simple transverse fracture of the patella. The accident had occurred on December 9.

Condition on admission.—The knee-joint was much swollen and contained a considerable quantity of fluid. The patella was fractured transversely and the fragments were about two inches apart. There was a good deal of pain in the joint.

Operation.—On December 13, chloroform having been administered, Mr. Lister made a longitudinal incision about 4 inches in length over the patella, the various aseptic precautions before described being employed. The knee-joint was of course at once opened. A quantity of coagulated blood and fibrous tissue filled up the space between the fragments, and this was removed. A pair of dressing forceps was then passed into the joint and projected against the skin at the most dependent part of the outer side of the joint. An incision was made on the projecting point, and by means of the forceps a horse-hair drain was drawn into the joint. The fragments were

then drilled obliquely, avoiding the cartilage, and, a piece of strong silver wire being passed along the drill-tracks, the fragments were firmly tied together. The two ends of the wire were then twisted together and left of sufficient length to project from the wound. A drainage tube was introduced into the incision superficial to the patella and brought out of the upper angle of the wound. The remainder of the line of incision was stitched. A large antiseptic dressing was applied enveloping the whole of the thigh, and the limb was placed on a posterior splint. The foot of the bed was raised on blocks so as to make the discharge flow upwards.

After-progress.—The after-progress of this case was typical. There was never the slightest pain or constitutional disturbance (see Fig. 77). On the following day (December 15) the dress-

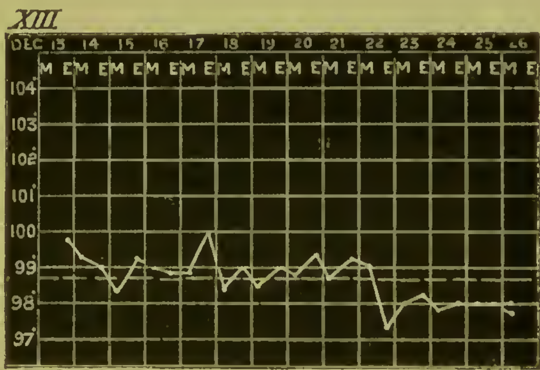


FIG. 77.—TEMPERATURE CHART FROM A CASE OF OPERATION FOR FRACTURE OF THE PATELLA. (No. 21, p. 434.)

ing was changed and a fresh dressing re-applied; there was no pain or swelling of the joint. On December 17, the dressing was again changed, and a portion of the drain was removed from the joint. On December 19, the superficial drain and part of the remaining drain for the joint were dispensed with; most of the stitches were taken out. At the next dressing, on December 24, the remainder of the drain and the rest of the stitches were removed. On December 31, the wound was again dressed, and was found to be quite healed, except a minute speck, where the drain for the joint had been. This was found to be quite healed on January 5, when passive motion was begun. The wire was removed on February 9, and bony union was found to have taken place. When the patient was discharged on February 15, the movements of the joint were perfect through an angle of 45°; patient could kick vigorously.

I. Wounds

No.	Name and Age	Date of Admission, Operation, and Discharge; with Result	Injury
1	Frank K., 29 .	*Ad.*, Nov. 20, 1871. *Op.*, Nov. 20, ,, *Dis.*, Dec. 23, ,, *Result*, cured.	Little finger-joint fractured in various places. Skin over hand contused. Fourth metacarpal bone laid bare in the greater part of its extent. Fourth metacarpo-phalangeal joint opened.
2	George G., 60 .	*Ad.*, June 28, 1872. *Op.*, June 28, ,, *Dis.*, Oct. 1, ,, *Result*, cured.	Compound comminuted fracture of the humerus, caused by the wheel of a wagon passing over his arm. Humerus fractured in two places; the lower fracture communicating with the elbow-joint. Patient admitted about two hours and a half after the accident.
3	Robert H., 30 .	*Ad.*, May 12, 1873. *Op.*, May 12, ,, *Dis.*, July 17, ,, *Result*, failed; amputation.	Compound fracture of the carpal bones of the left hand. Extensive laceration of the soft parts. Machinery accident. Patient admitted immediately after the accident.
4	Walter S., 42 .	*Ad.*, May 17, 1875. *Op.*, May 17, ,, *Dis.*, May 21, ,, *Result*, in process of cure.	Wound of metacarpo-phalangeal joint.
5	Francis J., 48 .	*Ad.*, Oct. 4, 1875. *Op.*, Oct. 4, ,, *Dis.*, Dec. 4, ,, *Result*, cured.	Compound dislocation of the ankle: the articular surface of the tibia protruding through a large wound anteriorly. Both malleoli torn off.
6	Henry W. . .	*Ad.*, June 8, 1876. *Op.*, June 8, ,, *Dis.*, June 13, ,, *Result*, cured.	Thumb and trapezium nearly torn off, and the carpal joints opened. Gunshot-wound.
7	David S., 13 .	*Ad.*, May 2, 1877. *Op.*, May 2, ,, *Dis.*, after Mr. Lister left Edinburgh. *Result*, cured.	Large lacerated wound of right knee. Large flap of skin thrown to one side. Mud was ground into the cartilaginous surface of the internal condyle of the femur. The accident resulted from a wheel of a heavy cart passing over patient's leg.

) JOINTS.

Treatment	Remarks
.tle finger amputated; other parts ·ell syringed with 1-20 carbolic lotion.	Some sloughing and suppuration occurred among the contused parts in the hand, and an abscess formed in the forearm. Entirely healed December 23. Passive movements were begun in December, and could be easily performed.
und injected with 1-20 carbolic lotion. ome loose pieces of bone were removed ·om the lower wound. (No portion f the articular end of the humerus ·as, however, removed.)	Putrefaction was avoided; typical aseptic course. The fracture had quite united on August 10. The wound was quite superficial on August 3, and boracic dressing was applied. On August 15, erysipelas attacked the wound. This passed off, and the wounds were quite healed on September 20. When dismissed, patient was able to flex his arm sufficiently to enable him to touch the shoulder of the other side.
und injected with a solution of carolic acid in rectified spirit (1 in 5).	Putrefaction occurred in spite of the injection; and, as fever set in, Mr. Lister operated on May 16, removing the carpus and fingers, but leaving the trapezium and the thumb. The flaps were left gaping, and carbolic oiled lint (1-10) was introduced between them. Flaps brought together on May 24. Stump was quite healed on June 25.
shed out with 1-20 carbolic lotion.	Putrefaction was avoided. Treated as an out-patient. Wound followed an aseptic course.
: wound was injected with 1-20 carolic lotion, and an attempt was then 1ade to reduce the dislocation. This iiling, the articular surface of the ibia, was sawn off, the astragalus eing left untouched. Foot fixed at ight angles to leg.	Aseptic course. The wound was almost absolutely healed when the patient was discharged. Joint strong and slightly movable. The temperature was on one occasion as high as 100 deg. F.
: thumb, with its metacarpal bone, ·as removed; the trapezium was also issected out. The wound and the arpal joints, as far as possible, were ıjected with 1-5 solution of carbolic cid in rectified spirit.	Aseptic course. Treated as an out-patient after June 13. On June 31, the wound had almost entirely healed.
eds of tissue were clipped away, and he cartilage of the condyle was pared ·ith a knife where the dirt was most round in. The whole of the dirty ·ound was scrubbed with a nail-brush, nd 1-20 carbolic lotion, and, in addiion, 1-5 spirituous solution of carbolic cid were applied. No stitches were nserted. The limb was placed on a osterior splint.	The wound became filled with blood-clot, the deeper part of which became organised. On May 27, there was a large granulating surface. The wound was quite superficial on June 26, when boracic dressing was substituted for the carbolic acid. There was at that time very considerable movement of the knee-joint, without pain. In August 1878, 'patient visited the hospital, walking without any assistance, the two knees being equally useful, except that the injured one was still somewhat stiff.'

I. Wounds...

No.	Name and Age	Date of Admission, Operation, and Discharge; with Result	Injury
8	Ellen M., 12 .	*Ad.*, Nov. 6, 1877. *Op.*, Nov. 6, „ *Dis.*, Nov. 17, „ *Result*, cured.	Punctured wound of ankle-joint, caused by scissors. The accident happened twenty-one hours before admission. Glairy fluid escaped, and a probe passed into the joint. Foot red and swollen.
9	Jane D., 50 . .	*Ad.*, Oct. 15, 1878. *Op.*, Oct 15, „ *Dis.*, June 20, 1879. *Result*, wound of joint cured.	Compound fracture of the lower end of the femur, with splintering of the condyles into the joint. Patient was seen one hour and a half after the accident. (See compound fracture, No. 70.)
10	Maria L., 60 .	*Ad.*, Nov. 12, 1878. *Op.*, Nov. 12, „ *Dis.*, April 19, 1879. *Result*, in process of cure.	Compound dislocation of the left ankle-joint, with comminuted fracture of the fibula and fracture of the internal malleolus. Skin in the neighbourhood of the wound much contused.
11	Henry B., 22 .	*Ad.*, April 20, 1879. *Op.*, April 21, „ *Dis.*, May 30, to come as out-patient. *Result*, cured.	Patient jumped over Waterloo Bridge; in his descent, he struck his left elbow against the side of the parapet. The result was an oblique fracture into the elbow-joint, detaching the internal condyle. There was a small opening in the skin communicating with the fracture.
12	Samuel M., 54 .	*Ad.*, July 8, 1879. *Op.*, July 8, „ *Dis.*, July 24, „ *Result*, cured.	Punctured wound of the left knee-joint, just above the patella. The finger, when introduced into the wound, passed into the joint, and felt the under surface of the patella. Synovial fluid escaped. Great pain on movement of the joint. The wound was inflicted about fourteen hours before the patient came to the hospital.
13	Archibald R., 16	*Ad.*, Dec. 10, 1880. *Dis.*, Dec. 24, „ *Result*, cured.	Incised wound of knee-joint. Incision one and a half inch long.
14	Ann P., 68 . .	*Ad.*, Aug. 11, 1881. *Op.*, Aug. 12, „ *Dis.*, Aug. 30, „ *Result*, cured.	Compound fracture of the olecranon. The accident happened on August 10, and was immediately seen by the house surgeon.

' JOINTS (*continued*).

Treatment	Remarks
Wound enlarged, and joint injected with a solution of carbolic acid in spirit (1–5).	The wound had quite healed on November 17. Aseptic course; dressed four times. When patient was discharged, the ankle was quite normal, with perfect movement. (See Chart I.)
The opening in the skin was enlarged. The projecting end of the femur was sawn off, and reduction was effected. The wound was washed out with 1 to 20 carbolic lotion. An incision was made into the knee-joint on the outer side, and a drainage-tube was inserted into it, to prevent accumulation of fluid.	Aseptic course. The drainage-tube was removed from the joint on October 28, and the wound of the joint had completely healed on November 21: the wound in the thigh healed on December 13. As the fracture remained ununited, Mr. Lister injected iodine between the ends of the fragments on February 14. Union not yet occurring, Mr. Lister cut off the ends of the bones and wired them together with thick silver wire. The femur is still ununited, but is under treatment. (See Chart II.)
The detached portions of bone were removed; wound syringed out with 1–20 carbolic lotion. Dislocation reduced. Drainage-tubes inserted. Dupuytren's splint.	Aseptic course. Some portions of the skin sloughed. When discharged, the wound was almost healed, but the ankle-joint was stiff.
Wound enlarged; some small fragments of bone removed, and the wound and joint syringed out with 1–20 carbolic lotion. Drainage-tubes inserted.	Aseptic course. Wound completely healed on June 30. The movements of the joint were then very good, and have since that time steadily improved. (See Chart III.)
Joint washed out with 1–20 carbolic lotion, and with a solution of carbolic acid in rectified spirit (1–5). Drainage-tube inserted; posterior splint applied.	Aseptic course. The pain on moving the knee ceased a few hours after it had been washed out. The wound was quite superficial when the patient was discharged, and the knee was quite movable. Healing was complete on July 31. The patient was again seen in October, the movements of the knee being then perfect.
Wound syringed out with an emulsion of eucalyptus oil containing iodoform. Surrounding parts washed with 1-20 carbolic lotion. Tube introduced into the joint.	Typical aseptic course. Tube removed on December 17. When discharged the wound had almost entirely healed, and healing was complete in a few days. Knee-joint perfectly movable and leg quite strong.
On the 10th the wound was thoroughly washed out with 1–20 lotion and treated aseptically. As there was no bed, patient could not be admitted till the following day. On the 12th the wound was again washed out, and, the opening being enlarged, the fragments were brought together by means of strong silver wire. Drainage-tube inserted, wound stitched, straight splint applied anteriorly.	Aseptic course. Some of the stitches were too tight and caused a little irritation, but this subsided as soon as they were cut. Incision healed and stitches removed on August 16. Drains entirely removed on August 19. The wound had quite healed when the patient was discharged. (The ends of the wire which brought the fragments together were cut short, and the cutaneous margins were brought together over them, so that the wire remains.) Seen in middle of September—extension perfect. Flexion beyond a right angle, and can be done perfectly by passive motion. Arm gaining strength.

II.—OPERATIONS ON

No.	Name and Age	Date of Admission, Operation, and Discharge; with Result	Disease
1	John C., 46 . .	*Ad.*, April 5, 1872. *Op.*, April 6, „ *Dis.*, May 9, „ *Result*, cured.	Caries of metacarpal bone and first phalanx of ring-finger.
2	David B., 39 .	*Ad.*, July 22, 1872. *Op.*, July 22, „ *Dis.*, Aug. 5, „ *Result*, cured.	Loose cartilage in knee-joint.
3	John McI., 34 .	*Ad.*, March 13, 1873. *Op.*, March 28, „ *Dis.*, June 6, „ *Result*, cured.	Ununited fracture of olecranon; fracture oblique; considerable separation of fragments. Patient unable to extend the arm. Accident occurred five months previously.
4	John H. 19 .	*Ad.*, May 6, 1873. *Op.*, May 31, „ *Dis.*, Aug. 15, „ *Result*, cured.	Dislocation of the lower end of the ulna backwards. The lower end of the radius was much thickened. Suppuration had occurred in the sheaths of the flexor tendons. The movements of the wrist-joint were painless, but limited, more especially as regards extension.
5	William T., 31.	*Ad.*, Nov. 19, 1873. *Op.*, Nov. 26, „ *Dis.*, Jan. 8, 1874. *Result*, cured.	Loose cartilages in the elbow-joint. The movements of flexion and extension were impaired, more especially the former. Pain on attempting to complete these movements.
6	Frances G., 54 .	*Ad.*, Nov. 3, 1873. *Op.*, Feb. 8, 1874. *Dis.*, Sep. 14, „ *Result*, improved.	Ununited fracture of neck of right femur. Accident happened 18 months before admission. Right limb, $29\frac{5}{8}$ in.; left limb, $31\frac{1}{2}$ in. Suffered great pain; could neither sit nor walk.

EALTHY JOINTS.

Treatment	Remarks
Removal of the finger and the whole of the metacarpal bone; the carpal articulations being of course opened.	On April 23, the wound had entirely healed except a small point at the distal end. Aseptic course.
Free incision into joint; cartilage removed.	Wound was completely healed on August 5. It had been dressed four times. Aseptic course. Knee freely movable.
Longitudinal incision over olecranon, the cartilaginous end of the humerus being at once freely exposed. Ends of fragments refreshed, drilled, and tied together by strong silver wire. Wound left open. Splint applied so as to keep the arm extended.	The wound had completely healed on May 2, except where the wire projected. Dressed ten times. Wire removed on May 19, when union was complete. Passive motion was commenced on April 8, and was performed at each dressing. Typical aseptic course. When dismissed, the movements of the arm were almost perfect. In a letter received from the patient some time afterwards, he stated that the one arm was as good as the other. The temperature in this case was irregularly taken but it was only once above 100° F., and then it was 100·6° F., an evening temperature.
Abscesses opened; end of ulna removed; drainage-tube introduced into the wrist-joint, which was healthy.	No local or constitutional disturbance followed the operation. When sent to the convalescent home on August 15, there was still a small sinus, but this soon healed. The movements of the wrist-joint were greatly improved.
Longitudinal incision over external condyle. Joint opened; loose bodies (about 200 in number) scooped out. Two drainage-tubes were inserted; wound stitched.	Healed on December 17. Dressed seven times. Aseptic course. Arm was paralysed at first, owing to the pressure of the tourniquet; but, under the use of galvanism, this was cured. When dismissed, the movements were much improved. (See paper by Mr. Sampson Gamgee in *Lancet* for January 10, 1874.) The temperature in this case was only taken once daily, and was only once above 99° F., viz. on the morning after the operation, when it was 99·7° F.
The limb having been drawn down to full length by pulleys, an incision was made over and above the trochanter, and the ends of the fragments were refreshed with the gouge and hammer, the joint being opened in the process. Drainage-tubes inserted into joint; no stitches; long splint and extension with weight and pulley applied. Length of right leg after operation, 30¾ in.	Healed on March 28: aseptic course. Extension was maintained till April 3; but when it was at length removed, it was discovered that the weight had been too heavy, and that the limb operated on was longer than the other. When the patient left the hospital, there was not osseous union, but she was able to walk fairly and sit, and the pain which she previously suffered had completely disappeared. (See Chart IV.)

II.—OPERATIONS ON

No.	Name and Age	Date of Admission, Operation, and Discharge; with Result	Disease
7	Grace S., 15 .	*Ad.*, May 6, 1874. *Op.*, May 14, „ *Dis.*, May 27, „ *Result*, cured.	Ganglion on the back of both wrists beneath the extensor tendons, which had resisted all treatment; multilocular.
8	Agnes ——, 17	*Ad.*, July 1875. *Op.*, July 13, 1875. *Dis.*, Oct. 20, „ *Result*, cured.	Ostitis of the tibia; bone much thickened. A sinus was present, leading down to bare bone.
9	Edward R., 59 .	*Ad.*, Aug. 12, 1875. *Op.*, Aug. 17, „ *Dis.*, Dec. 16, „ *Result*, cured.	Enchondroma of scapula. A tumour had been removed from the same region seven years before the patient's admission to hospital.
10	Alexander—, 26	*Ad.*, Nov. 10, 1875. *Op.*, Nov. 16, „ *Dis.*, Dec. 26, „ *Result*, cured.	Four months before admission, patient met with an accident, causing fracture of the ulna a little above its middle, and dislocation of the head of radius backwards. Pronation and supination were almost impossible. Extension could be carried slightly beyond a right angle.
11	James D., 16 .	*Ad.*, Feb. 14, 1876. *Op.*, Feb. 14 „ *Dis.*, Mar. 20, „ *Result*, cured.	Patient was run over immediately before admission. Left ankle much bruised and distended with effused blood.
12	John D., 64 . .	*Ad.*, May 18, 1877. *Op.*, June, 1 „ *Dis.*, Sept. 8, „ *Result*, much improved.	Rupture of rectus femoris and crureus in both thighs, the vasti being still attached to the sides of the patella. No power of extension; though, if the leg were extended, it could be kept so. If it became at all bent, the patient fell.

EALTHY JOINTS (*continued*).

Treatment	Remarks
oth ganglia were removed, the tendons being exposed during the operation and held aside. On the right side the wrist-joint was opened, the articular surfaces of the scaphoid and radius being seen.	When dismissed to be treated as an out-patient healing was not complete. On June 2, the left wrist was found healed; on June 7, the right had also healed. Left dressed seven times; right eight. Drainage-tubes removed May 20. Typical aseptic course. Movements of fingers and wrist-joints on both sides were perfect. Temperature irregularly taken. Highest temperature was 99·5° F.
'hinking that the case was one of necrosis, Mr. Lister cut down and gouged out a portion of the bone. In doing so, he found the interior of the shaft softened, and converted into a sort of granulation material. In scraping out this matter, the gouge accidentally passed into the ankle-joint, which was healthy. Chloride of zinc was applied, and a drainage-tube inserted.	When the patient was discharged, the wound had completely healed. There was no pain, and the ankle-joint was freely movable. No constitutional or local disturbance followed the operation.
'he whole of the scapula below the spine, the spine itself, and about one-third of the glenoid cavity were removed, the upper border of the scapula and part of the glenoid fossa being left.	A portion of one of the flaps lost its vitality, leaving a large deep hole, at the bottom of which the articular end of the humerus could be seen. This took a long time to fill up, but was completely healed when the patient was discharged. The rest of the wound healed by first intention. Passive movement was kept up, and when the patient was dismissed, there was good movement at the shoulder-joint. (See Chart V.)
External lateral ligament of the elbow-joint divided and head of radius snipped off. Drainage-tube inserted into joint. No stitches.	Healing was complete on December 8. Dressed six times; aseptic course. Passive movements were begun on November 18. When discharged, the movements in all directions were very fair. When seen again in September 1877, all the movements were almost absolutely perfect.
'oint incised and clots evacuated.	Aseptic course. On March 10, the wound was quite superficial, and boracic dressing was applied. Movements of joint normal.
The vasti were detached from the sides of the patella. The upper border of the patella was rawed, and the vasti were stitched to it—a V-shaped portion being taken out of the rectus and crurens. Counter-openings made on each side of the joint and drainage-tubes inserted. Only one knee operated on.	Wound went on well for about three weeks, when, owing to the patient pushing his hand under the dressings, the wound putrefied; but by this time the opening into the joint had closed, and no harm resulted. When dismissed, the patient could extend his knee after it had been bent to an angle of 135°; and the other knee being kept extended by means of a splint, he could walk without fear of falling. (See report of case by Dr. Roxburgh in *Lancet*, 1878.) (See Chart VI.)

II.—OPERATIONS ON

No.	Name and Age	Date of Admission, Operation, and Discharge; with Result	Disease
13	Adam W., 7 .	*Ad.*, May 27, 1877. *Op.*, July 6, ,, *Dis.*, Aug. 24, ,, *Result*, cured.	Badly united fracture of the lower end of the humerus, the lower part of the upper fragment projecting backwards and locking the olecranon, thus causing inability to extend the forearm beyond an angle of 120°. Of some months' standing.
14	William T., 35 (See No. 5.)	*Ad.*, July 19, 1877. *Op.*, July 25, ,, *Dis.*, Aug. 11, ,, *Result*, cured.	Return of previous symptoms.
15	Francis S., 40 .	*Ad.*, Oct. 24, 1877. *Op.*, Oct. 26, ,, *Dis.*, Jan. 11, 1878. *Result*, cured.	Transverse fracture of the right patella, produced by striking the bent knee against a bar. Patient was admitted on October 12, but insisted on going home, and on being treated by apparatus. As the apparatus did not answer, he now readily consented to the performance of the operation previously proposed.
16	John S., 5	*Ad.*, Feb. 6, 1878. *Op.*, Feb. 6, ,, *Dis.*, Mar. 29, ,, *Result*, cured.	Hæmophilia, with effusion of blood into the right ankle-joint. Stated that he was kicked on the ankle three days before admission. Patient was suffering great pain and constitutional disturbance. The joint was tense from the presence of fluid, and, as Mr. Lister feared that suppuration might ensue, he incised the joint.

Healthy Joints (*continued*).

Treatment	Remarks
Two longitudinal incisions made on the posterior aspect of the joint: one between the external condyle and the olecranon, the other between the olecranon and the ulnar nerve; these incisions, of course, opening the articulation. The projecting portion of the humerus was then removed, the attachment of the triceps to the olecranon process being left intact, and a hollow was gouged for the reception of the olecranon process. In order to get complete extension, it was necessary to remove the whole of the external condyle; drainage-tubes inserted.	The wounds had quite healed on July 30; dressed six times; typical aseptic course. When discharged, the movements of the elbow-joint were almost absolutely perfect. (See Chart VII.)
Elbow-joint opened; no loose cartilages found, but a number of bodies attached to a fringe of synovial membrane were removed.	Aseptic course. Wound had healed, and the movements were restored when the patient was discharged. (See Chart VIII.)
A longitudinal incision about three inches in length was made over the patella, when it appeared that the fragments were mutually displaced, and a mass of firm coagulum, mixed with fibrous tissue, interposed between them, so that it would have been impossible to bring the osseous surfaces into contact except by operative means. This material was removed, and the ends of the fragments were then refreshed, drilled obliquely and tied together with strong silver wire. An opening was made into the joint on the outer side for the introduction of a horsehair-drain. The wound was closed by stitches, and the limb placed on a posterior Gooch's splint.	The wound had completely healed on November 17, except where the wire was. Drain removed from knee on November 4. Incision on outer side of knee healed on November 11. Dressed seven times; typical aseptic course. Wire was removed on December 21. The splint was left off on January 7. Patella firmly united. There was naturally considerable stiffness of the knee, owing to the limb having lain so long in the splint, but it could be moved with ease through an angle of about 45 deg. Patient would not submit to forcible movement under chloroform, or even to wear an elastic apparatus for gradual flexion. (See Chart IX.)
n incision was made on each side of the ankle-joint. A quantity of dark fluid blood escaped. Horsehair-drain passed through the joint.	Both wounds had healed on March 22. Hæmorrhage occurred from one of the wounds on the morning after the operation, and recurred at intervals during three days, being at length checked by the application of a solution of perchloride of iron in glycerine. Wounds were quite superficial on March 12, and boracic dressing was therefore applied. No suppuration; aseptic course. When discharged, there was no pain in the joint: the joint was quite movable, and the boy was able to run about. Patient was in hospital in September 1879, on account of bleeding from his finger; the ankle was then in every respect quite normal. (See Chart X.)

II.—OPERATIONS ON

No.	Name and Age	Date of Admission, Operation, and Discharge; with Result	Disease
17	William B., 45 .	*Ad.*, March 18, 1879. *Op.*, March 20, ,, *Dis.*, May 4, ,, *Result*, cured.	Ununited fracture of the olecranon of nine weeks' standing. Patient could only imperfectly extend his forearm. The fracture was oblique, and, on flexion, there was considerable separation of the fragments.
18	Edward W., 12	*Ad.*, Jan. 5, 1879. *Op.*, Jan. 15, ,, *Dis.*, June 12, ,, *Result*, in process of cure.	Badly united fracture of the lower end of the humerus, with dislocation of both bones of the forearm backward. Accident happened three months before admission. The movements of the elbow-joint were very limited.
19	James P., 34 .	*Ad.*, March 19, 1879. *Op.*, March 24, ,, *Dis.*, April 25, ,, *Result*, right cured; left *in statu quo*.	Loose cartilages in both knee-joints.

HEALTHY JOINTS (*continued*).

Treatment	Remarks
An incision was made on the ulnar side of the posterior surface of the olecranon. Ends of fragments were refreshed with a chisel and hammer, the joint being of course opened. Fragments were then drilled obliquely and tied together with strong silver wire. Horsehair-drain passed into joint. Wound stitched; arm placed on a splint in the extended position.	The wound had entirely healed on April 9, except where the wire was. Typical aseptic course. Drain and stitches were removed on March 27. Passive motion was begun on March 31; wire removed May 11, the union of the fragments being then complete. When discharged, he was able to extend his arm almost completely. In a letter received from him in October 1878, he states that his arm is almost as useful as the other, and that he can carry on his trade as a plasterer, which he could not do before the operation. (See Chart XI.)
On the supposition that the case was simply one of fracture, a longitudinal incision was made behind the joint, with the intention of excising it; but the true nature of the case being revealed, it was determined to avoid interference with the bones of the forearm. A small slice was sawn away from the lower end of the humerus, which was greatly distorted and thickened by callus. The lower end of the humerus was then pared and shaped with chisel and gouge, so as to resemble the natural form of the articular end of the bone, hollows being gouged for the reception of the coronoid and olecranon processes. The dislocation was then reduced; drainage-tubes inserted, and wound stitched. The reason for preferring this operation to complete excision was to avoid the lagging behind in growth of the forearm and hand, which is so apt to occur after that operation in young children.	Aseptic course. Passive motion was begun on the day after the operation. Pronation and supination were perfect from the first, and always continued so. Owing to the movements, a sore remained over the olecranon till April 10, when it had completely healed. The limb was very strong. The movements of extension and flexion were fair, and were constantly improving when the patient was discharged.
Having fixed the cartilage in the right knee, Mr. Lister cut down and removed it. Drainage-tube put into joint. Two days afterwards, a free incision was made into the other joint, in search of a very small loose cartilage, which could not be fixed, but which the patient could generally bring by his own manipulations to the spot incised. Protracted manipulations on his own part, carried out under the spray with carbolised hands, failed however to bring the body to the wound; nor could it be discovered, after a long search, with the finger and hooks.	Both wounds followed an aseptic course. Drainage-tubes removed from each the day after the operation. Right knee healed April 12; left knee had healed before the patient left the hospital. Movements on both sides unimpaired.

II.—OPERATIONS ON

No.	Name and Age	Date of Admission, Operation, and Discharge; with Result	Disease
20	Andrew G., 28 .	*Ad.*, June 6, 1879. *Op.*, June 6, ,, *Dis.*, July 11, ,, *Result*, cured.	Patient was a medical man. He had suffered from bunion, beside the metatarso-phalangeal joint of the great toe of the right foot, for 14 years. Pads, etc., had been used, but without effect. Patient was unable to walk even short distances without great pain, and he could not enter on the practice for which his medical education had now qualified him.
21	William T., 37 .	*Ad.*, Dec. 13, 1879. *Op.*, Dec. 15, ,, *Dis.*, Feb. 15, 1880. *Result*, cured.	Recent simple transverse fracture of the patella. The accident happened on Dec. 9.
22	James K., 29 .	*Ad.*, April 10, 1880. *Op.*, April 12, ,, *Dis.*, Sep. 25, ,, *Result*, stiff knee.	Loose cartilage in the left knee joint.
23	Joseph R., 24 .	*Ad.*, Sep. 27, 1880. *Op.*, Oct. 22, ,, (See No. 25).	Ununited fracture of the patella of several months standing. Upper fragment pretty firmly adherent to the anterior surface of the femur. Lower fragment very small. Patient cannot walk at all.

HEALTHY JOINTS (*continued*).

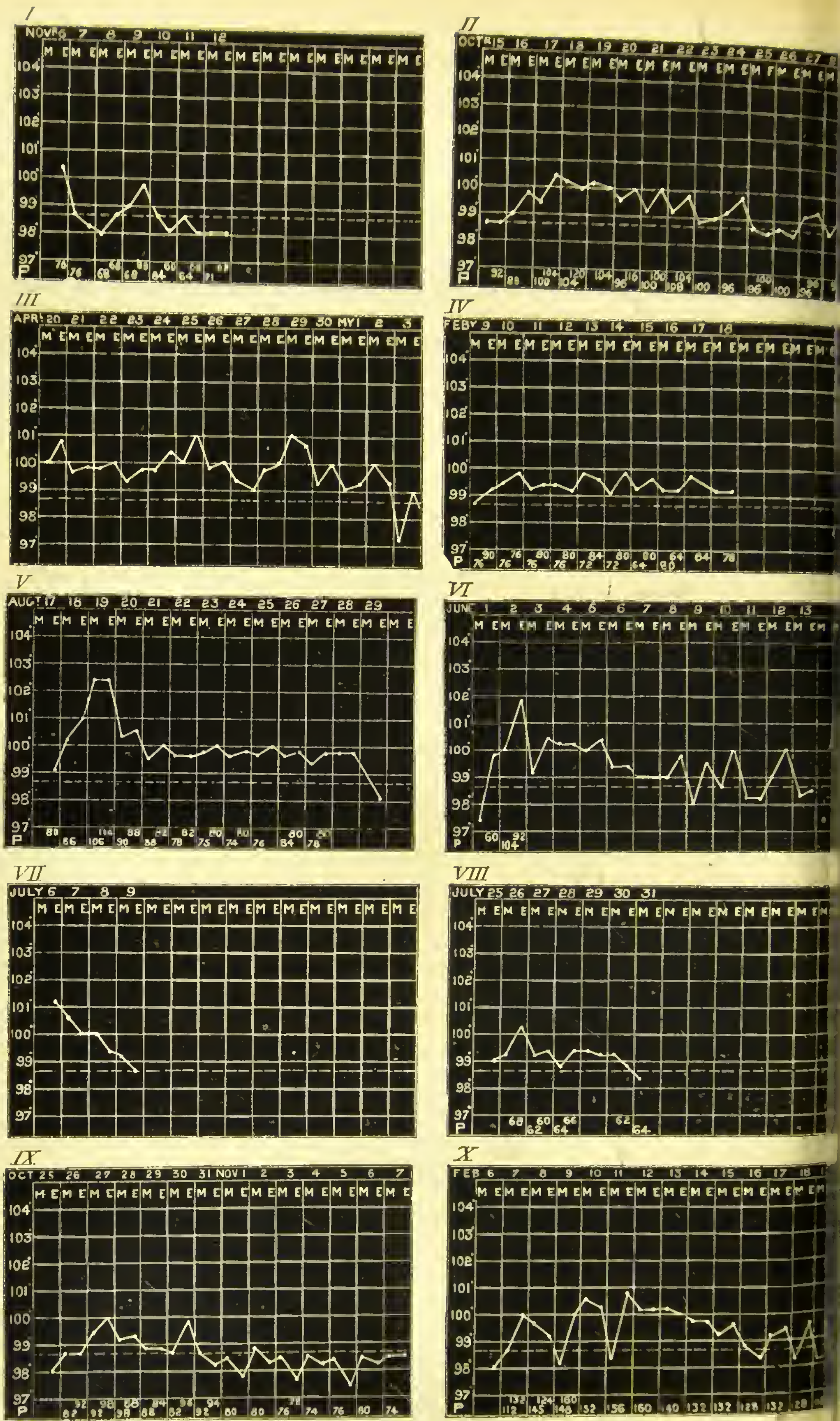
Treatment	Remarks
Mr. Lister made a longitudinal incision over the inner side of the joint on the dorsal aspect. The joint was opened, and the projecting inner end of the extremity of the metatarsal bone cut off. The remainder of the joint left intact. Drainage-tube inserted ; no stitches.	Aseptic course. Wound was quite healed on July 9. When discharged, the joint was quite movable and the swelling from thickening of the soft parts much less. In a note received from him on September 16, 1879, patient writes : 'The prominence on the inner side of the foot has entirely disappeared, and I have perfect use of the joint, with entire absence of pain. I can walk ten miles without any inconvenience.' (See Chart XII.)
See description of operation, p. 420.	Typical course. See p. 420. When the patient was discharged the movements of the knee-joint were perfect through an angle of 45°, and were daily improving. Patient could kick vigorously. (See Chart XIII.)
The cartilage was removed by a free incision, and a drainage-tube was inserted into the joint.	On the evening of the day of the operation the knee was very painful, and there was so much discharge that it was necessary to change the small dressing which had been applied at the time of the operation. On the following morning the temperature was up to 102·8° F., the knee was swollen and very painful, and the dressings were saturated with discharge. There was a blush of redness around the wound. The discharge had no smell. This state of matters got worse, the temperature went still higher, and for several days varied between 101° and 103·6°. The discharge from the joint became purulent and an incision was made on the outer side of the patella into the joint. An abscess formed in the thigh, and was opened. During May matters began to improve, and gradually the discharge diminished and the various wounds closed. All had healed on September 25. The knee was almost stiff, but it was possible to get slight movement (fibrous anchylosis). The patient was advised to return in a month to have the adhesions broken down, but he did not do so. (See Chart XV.)
Longitudinal incision over the front of the joint. Upper fragment detached from the femur. Ends of fragments refreshed and tied together with silver wire. Drainage as in the former cases.	Aseptic course. Stitches removed on October 25. The wounds had completely healed on November 4. On November 20 the patient was put under chloroform, and an attempt made to bend the limb. The upper fragment had, however, again become adherent to the femur, and in attempting to bend the limb the wire broke, and the fragments became separated. The case was re-operated on on November 26 (see No. 25). (See Chart XIV.)

II. Operations on

No.	Name and Age	Date of Admission, Operation, and Discharge; with result	Disease
24	Martha F., 43 .	*Ad.*, Nov. 5, 1880. *Op.*, Nov. 12, ,, *Result*, cured.	Fracture of patella of eight weeks' standing. Patient walks with great difficulty and with the aid of sticks.
25	Joseph R., 24 .	See No. 23. *Op.*, Nov. 26, 1880. *Dis.*, Feb. 5, 1881. *Result*, cured.	See No. 23. Refractured patella.
26	William G., 62.	*Ad.*, June 21, 1881. *Op.*, June 24, 1881. *Dis.*, Aug. 11, 1881. *Result*, cured.	Recent fracture of patella. Accident happened on June 21.

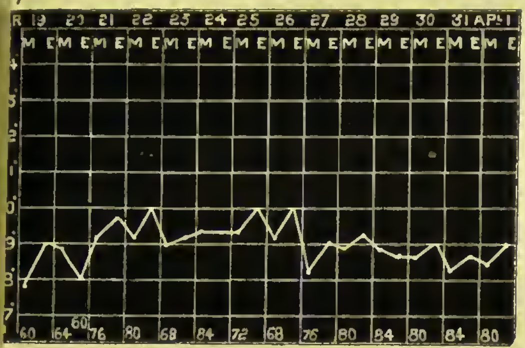
HEALTHY JOINTS (*continued*).

Operation	Remarks
Operated on in the same manner as the others. A drainage-tube used for the joint instead of a horse-hair drain.	Aseptic course. No pain, inflammation or constitutional disturbance. Drainage-tubes removed November 16. Almost healed on November 22. Quite healed on November 27. Seen on May 31, 1881. Could then walk comfortably without a stick, could raise the leg from the bed and flex the knee, but the flexion could not be carried quite to a right angle. (See Chart XVI.)
Re-operated on as before. No drainage of the joint was necessary. Wound left open in part.	Aseptic course. On December 8 everything had healed except a small part of the gaping incision where blood-clot filled the wound. On scraping away the superficial layer of this clot, cicatrisation was found to have advanced considerably beneath it. Seen in April 1881; could walk comfortably with a stick. Could not raise leg from bed, but was able to bring it forward in walking. Flexion gradually improving. Union of fragments good; no separation. (See Chart XVII.)
Treated like No. 21.	Aseptic course. On the morning of the 24th (before the operation) the temperature was 100·2°, in the evening 100·6°. On the 25th T. M. 100°, E. 100°; dressed. 26th, T. M. 100·2° E., 98·6°; dressed. The drain was not acting very well, which probably accounts for the continued elevation of temperature on the 25th; as soon as that was put right the T. fell. On the 27th T. M. 99·4°, E. 100·4°. On the 28th T. M. 99·8, E. 99·6. On the 29th tube left out. Dressed again on July 6. Almost entirely healed and spray stopped on July 9. A minute spot unhealed on July 15. The temperature after the 28th ranged between 98·8° and 99·8°. Passive motion could be readily performed without any pain.

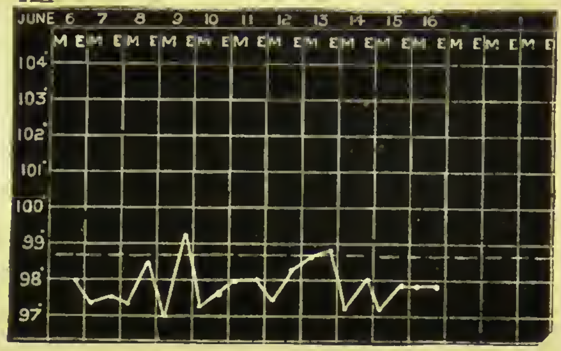


TEMPERATURE CHARTS OF MR. LISTER'S CAS

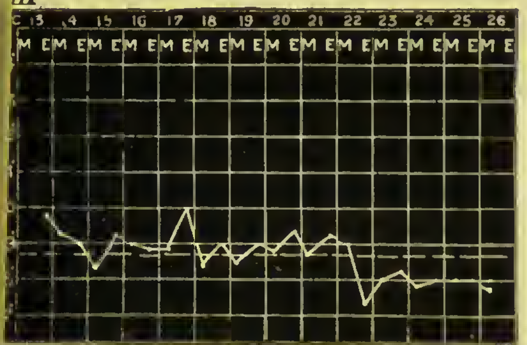
I



XII



III



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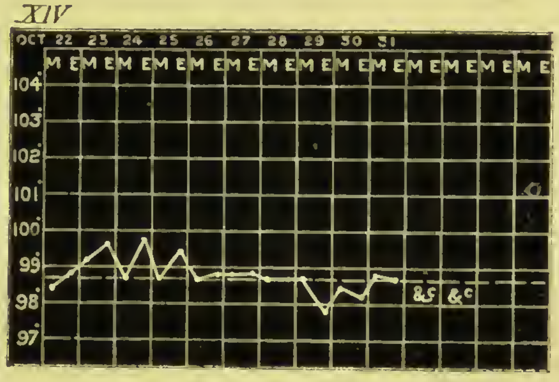
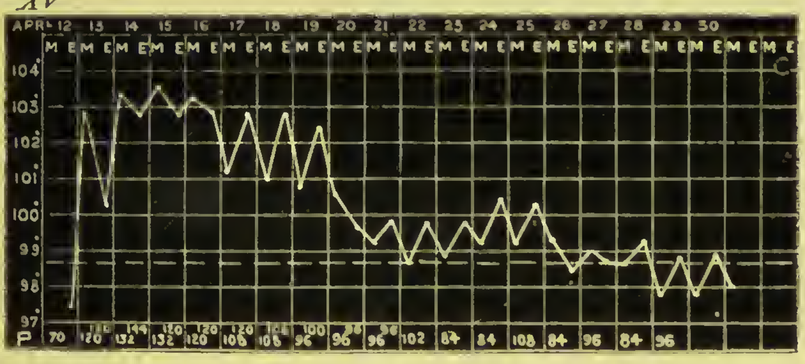
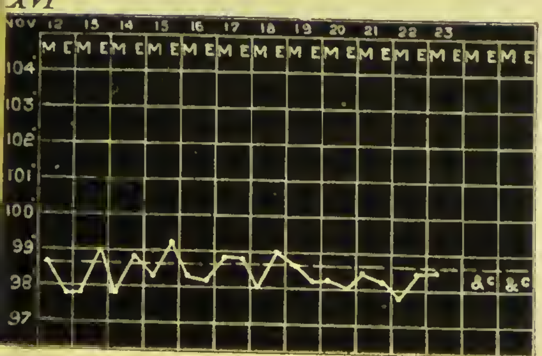


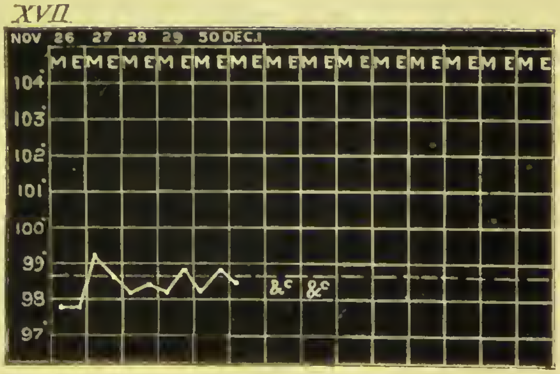
XV



XVI



XVII.



F Wounds of, and Operations on, Joints.

The result, then, is that Mr. Lister has since the end of 1871 treated conservatively in hospital 40 cases of injuries of healthy joints without a death, and in only one case (wounds of joints, No. 3), was there any necessity for further operation. This was a case of wound of joint not made by the surgeon. In only one case in which the joint was opened by Mr. Lister did suppuration occur (No. 22), and here, as I shall presently shew, the aseptic method had been imperfectly carried out.

It has been objected to these cases by Mr. Bryant,[1] that an equally good series of cases might be found in the case-books of any hospital. It may indeed be that in some instances equally good results have been obtained, but I venture to affirm that no hospital case-book, recording cases not treated aseptically, would show that *all the cases treated in succession* had like results. It is as an integral record of a number of cases in succession rather than as a record of each individual case that these facts are important. There will no doubt be found throughout the journals isolated examples of remarkable results after injuries of joints, but then only the successful cases are published; we do not have a record of *all* the cases in the practice of a particular surgeon; we do not hear of the many cases where failure occurred, or where conservative treatment was not attempted. Then, again, the cases ordinarily recorded in journals have not been treated in the same way as Mr. Lister's. For Mr. Lister not only makes an incision into a joint, but he purposely keeps the communication open for some days by means of a drainage tube, and he does not syringe out the joint with any antiseptic solution. I do not believe that such a thing could be done with a poultice or other septic dressing without the occurrence of suppuration in the joint, and yet among Mr. Lister's 26 operation cases this only happened in one instance, and in it, as I shall presently point out, we have complete evidence that the requirements of the method had not been efficiently complied with.

Mr. Bryant also expressed his astonishment that Mr. Lister should regard the removal of foreign bodies from joints as a perfectly safe procedure on the evidence of 3 cases. But then it must be remembered that every case in which a joint was

[1] MacCormac's *Antiseptic Surgery*.

incised, for whatever purpose, proves the assertion that they may be opened for the removal of foreign bodies, and so at the time when Mr. Bryant spoke the evidence rested, not on 3 published cases, but on 20 cases of operations and 12 cases of accidental wounds, i.e., not on 3 but on 32 cases. And in reality it rested on many more, for this record only represents Mr. Lister's practice since the end of 1871. For five years previously Mr. Lister had been operating on joints in hospital with success, and the details of some of these cases have been published. And in private practice, also, during all these years Mr. Lister has performed a number of operations on healthy joints. Further, during the same time, many of Mr. Lister's pupils had performed similar operations with similar successes. Thus the evidence in support of Mr. Lister's statement is sufficiently ample to justify it.

It was further stated by Mr. Bryant, that a number of these cases were too trivial to be of value, but I venture to doubt the force of this argument. For I do not suppose that an incision into a carpal articulation will be regarded as a very trivial matter, and we have in the list only 3 cases where small joints were opened. But I do not regard wounds of phalangeal joints as such a very trivial injury after all. The cases of compound dislocation of the phalanges which I have seen treated conservatively but septically have almost all died. It may be that I have had an unlucky experience, but I have seen that amputation is in the great majority of cases performed for these injuries, and that where they are treated conservatively and without aseptic precautions a great risk is run. The only case of wound of a small joint not treated aseptically which has occurred to my knowledge at King's College Hospital during the last four years died of tetanus.

In speaking of ovariotomy I have said that it was not a test case, because the peritoneum absorbs fluid so quickly that the bacteria, if admitted, have no fluid in which they can develop; and I also added that the test case would be where there was ascites as well as an ovarian tumour, where, therefore, there was fluid in which organisms may develop. Such cases have always been regarded as particularly unfavourable for operation,

though surgeons do not as a rule seem to have understood the reason of this. A corresponding condition may be got in the case of joints, more especially in the knee joint in hydrops articuli. The ordinary practice in such cases, if they refuse to yield to the ordinary methods, is to remove the fluid by aspiration. That practice has been found to be perfectly safe; it is a subcutaneous, in fact an aseptic, operation. In Edinburgh in 2 cases of this kind (chronic synovitis), Mr. Lister made incisions into the joints and introduced a drainage tube. In neither case was there the slightest bad result. In the first case the drainage tube was removed on the fourteenth day, and the wound healed a few days later. In the second case the drainage tube was kept in a few days longer, and the wound had completely healed in five weeks. In both instances the disease was cured; in both the movements of the joint were perfect; in neither was there any suppuration, inflammation or constitutional disturbance. In King's College Hospital Mr. Lister has made incisions in 3 cases of acute synovitis verging on suppuration, and in all cases with cure without suppuration. In these cases the knee-joint was affected. In 2 the movements were good when the patient left the hospital. In the other movement is perfect, but the leg is kept in a silicate apparatus, as the ligaments have not yet regained their firmness, and the limb is apt to become over extended. (In this instance the inflammation was much more extensive, affecting the adjacent portions of the tibia and femur and producing softening of the ligaments.)

Then I must mention, that Mr. Lister's practice in cases of synovial disease of joints where the disease progresses in spite of rest, counter-irritants, &c., is to make free incisions into the joints, and introduce drainage tubes into them. This is done at once, without waiting for the formation of abscesses, in cases where the disease is progressing. In 16 cases so treated since 1871 no pus was present, and of these 16 cases 11, or 68·7 per cent., were cured without any further treatment (excision or amputation), and there never was any suppuration from the joint. In several of the cases considerable movement was obtained.

The cases were :—

8 incisions	into the	knee-joint	—no pus—	7	cured	without further	operation
4	,,	,,	tarsus	,,	2	,,	,,
2	,,	,,	elbow	,,	2	,,	,,
2	,,	,,	wrist	,,	0	,,	,,

None of the cases died.

Then, as I have said, where suppuration has occurred, the joint is also, in the first instance, simply incised and a drainage tube inserted; if necessary, further measures can be resorted to afterwards. Of 48 abscesses of joints so treated 27, or 56·3 per cent., were cured without further operation; and in these there was no more formation of pus after the incision.

These cases comprised:—

19 abscesses	of	the hip-joint—	13	cures	without	further	operation—	1	death
11	,,	,, knee	5	,,	,,		,,	1	,,
6	,,	,, tarsus	3	,,	,,		,,	1	,,
2	,,	,, shoulder	1	,,	,,		,,	—	,,
3	,,	,, elbow	1	,,	,,		,,	—	,,
7	,,	,, wrist	4	,,	,,		,,	—	,,

There were three deaths, all of them from tubercular meningitis, confirmed on post-mortem examination. One other case not included here must be mentioned. A patient, a little child, was admitted into King's College Hospital with pyæmia after scarlet fever. Abscesses were present in various joints, and as they were causing him considerable pain, they were opened. The child died two days later.

Thus, taking all the cases together, we have 109 instances where joints, healthy or more or less diseased, were opened and drainage tubes inserted; and of these 109 cases only 3 died, the cause of death in each instance being quite independent of the method of treatment adopted.

Leaving Mr. Lister's practice we find records of a number of wounds of joints in the practice of other surgeons.

Volkmann, in his first report, mentions the occurrence of 7 wounds of joints and 2 incisions into joints for loose cartilages without a death. In his last report he adds 24 cases of penetrating wounds of joints making in all 33 cases, distributed as follows, without a death.

	Cases	Deaths
Knee	15	0
Wrist	8	0
Elbow	5	0
Foot	5	0

No details of these cases are given, and we do not therefore know whether any further operative interference was necessary.

In his paper on compound fractures,[1] Volkmann adds the fact that 21 compound fractures passing into joints were treated without a death. With regard to these last 21 cases, we have some further particulars. In 2 instances resection was at once performed; in 5 resection was performed secondarily; in 3 amputation was performed secondarily; in 10 there was perfect recovery with freely movable joints; and in 1 case there was recovery with anchylosis, but the treatment was not commenced in this instance till thirteen days after the injury, and the joint was then already suppurating. In all these cases there was extensive injury of the bones as well as wound of the joint. In 3 cases the secondary operation was performed because the patient was not admitted for some time after the injury, and the joints were already suppurating; in one case the supervention of gangrene rendered it necessary, and in the other cases the nature of the wound, malposition of the fragments or suppuration of the joint, required it. Of course in considering cases of *wounds* of joints it must always be remembered, that one can never be certain that all the causes of putrefaction already present have been destroyed, and therefore, the probable result is very different from, *i.e.* much more uncertain than, that where the surgeon makes the wounds himself, and where, therefore, he has merely to exclude these causes. This is well illustrated in the case of compound fractures.

Max Schede, in his work on amputations, states that he has treated 15 cases of compound fracture in which joints were opened. Of these 10 were treated conservatively and healed, in 9 instances (5 of elbow, 2 of hand, and 2 of foot) with movable joints, in 1 with anchylosis, where the case was not admitted for some time, and where there was extensive com-

[1] 'Die Behandlung der complicirten Fracturen,' *Volkmann's Sammlung*, Nos. 117–118.

minution of the tibia, numerous portions of which necrosed. In 2 cases amputation was necessary on account of gangrene; in 1 case, which was not admitted till ten days after the injury, secondary amputation was performed; in 1 case of elbow injury a partial secondary resection was necessary, and in 1 case death occurred from trismus, the patient not having been admitted till the thirteenth day after the injury, when the joint was already suppurating.

Taken together with Volkmann's 54 cases we have 69 cases with 1 death. With Mr. Lister's 40 cases we have a total of 109 cases of injuries to or operations on healthy joints with 1 death, a death which ought not to be reckoned at all, for the patient was not treated aseptically from the beginning.

Both Volkmann and Schede mix up the cases in which treatment was commenced at once with those in which the patient was not admitted for some days after the injury, and where the joint was already suppurating. If we separate these cases from the 36 instances of compound fractures with wounds of joints, of which we have details, we find that 28 came under treatment within forty-eight hours after the occurrence of the accident, and of these 19 recovered with movable joints; in 2 cases primary and in 4 cases secondary resection was necessary, and in 3 secondary amputation was performed (twice for gangrene). On the other hand, of the 8 cases which did not come under treatment till forty-eight hours or more had elapsed, none recovered with movable joints, 2 recovered with stiff joints, 4 required secondary resection, 1 secondary amputation, and 1 died of tetanus. Of course in judging of the effects of the aseptic or other method of treatment in preventing the bad effects liable to follow wounds of joints, the latter class of cases, where treatment is not commenced for several days, ought to be excluded.

Paul Barth [1] published in 1877 the results of the cases of wounds of the knee-joint which had been treated in the hospital at Basel since 1873. They were 10 in number. Of these 9 recovered with movable knee-joints and one died of exhaustion. The latter was a case of compound fracture of the patella along

[1] *Ein Beitrag zur Behandlung der perforirenden Wunden des Kniegelenks*, by Paul Barth, Basel, 1877.

with injury to the right side of the head and fracture of the lower ends of both radii; the result of a fall from the first floor of a house to the street. For four days things seemed to be going on well, and then the patient began to complain of pain in the knee. The wound on the forehead began to suppurate, and the bone became bare. The patella also necrosed. An abscess formed in the thigh, and the pus had a foul smell. The patient sank, exhausted from the prolonged suppurations, about six months after the injury. In this case purification had not in all probability been successful in the first instance. Among these 10 cases we have 6 which were seen soon after the injury: the other 4 having come under treatment several days later when suppuration was commencing, or had already begun. The success in purifying the wounds in these 4 cases is remarkable; they all recovered with movable joints. In purifying them, the wound was swabbed out with chloride of zinc, and then irrigated with strong carbolic lotion, several counter-openings being also made. The successful disinfection in these cases was a piece of luck, for one can by no means reckon on anything like such a result.

Saxtorph of Copenhagen has published the results of his aseptic practice in removing foreign bodies from joints by free incision.[1] He has had 12 cases, 11 being cured and one ending fatally. In this case the patient took off the antiseptic dressing, suppuration ensued, and the patient died.

Piéchaud [2] has collected a number of cases of incisions into joints. He gives 4 cases of hydrarthrosis of the knee-joint which were treated by free incisions and aseptic drainage. They were operated on by Panas, Poinsot, Saxtorph and Lindpaintner. In 3 of the cases there was perfect cure with free movement of the joint. In the fourth case, in a strumous subject, there was some fear that synovial disease was going to develop, and therefore the patient was discharged wearing a silicate apparatus with which he could walk about without pain.

Piéchaud mentions 3 cases of dislocation of the thumb where, in order to reduce the dislocation, free incisions were

[1] *Clinique Chirurgicale.*

[2] *De la ponction et de l'incision dans les maladies articulaires.* Par le Dr. T. Piéchaud, Paris, 1880.

made into the joint. All recovered with retention of the normal movements. In a fourth case, the finger, which was otherwise much injured, became inflamed, and amputation was performed on the following day. Piéchaud also states, that in the course of the year he had seen in M. Labbe's clinique at the Lariboisière several similar cases where wounds of the phalangeal joints healed by first intention when treated aseptically.

Professor Nussbaum,[1] states in his work on aseptic treatment, that 'this method opens up a new field to surgeons. By the aid of these precautions, joints and the cavities of the body may be opened without danger.' This is his experience in an unhealthy hospital.

Professor Albert[2] of Innsbruck says; 'Die operative Eröffnung seröser und synovialer Hohlräumen die bei offener Wundbehandlung regelmässig zur Eiterung führt, führt unter Lister nicht zur Eiterung. Man kann Hydrocelen, Gelenke, Schleimbeutel ohne Gefahr eröffnen.'

Professor Hueter of Greifswald says; 'it is with reason that I said in 1870 that puncture of joints ought to be considered as a dangerous operation; it is with as much reason that I now affirm (1876) that one can practice this operation without danger. I based my first opinion on the septic accidents consecutive to the inflammation.'

M. Létiévant of Lyons also says:[3] 'Des tentatives opératoires nouvelles, très-graves, devant lesquelles on pouvait hésiter, ont pu être mises en application, et si je n'avais eu la sécurité que me donnent mes statisques et une pratique déjà longue du pansement listérien, je n'aurais jamais osé ouvrir largement des grandes articulations, les luxer pour les nettoyer, remettre les os en place, drainer et conduire la plaie à guérison.'

Kraske gives details of all the gunshot injuries of joints treated at Hallé aseptically.[4] They were 4 in number, and, in all the cases, the knee was the joint involved. In one case the movement was perfect, healing having taken place without

[1] *Le pansement antiseptique*, 1880.
[2] *Lehrbuch der Chirurgie.* Wien, 1877.
[3] Note sur le *Pansement antiseptique Listérien.* Lyon, 1880.
[4] Langenbeck's *Archiv*, vol. xxiv.

the occurrence of suppuration. In a second it was not absolutely perfect, but it was almost right, and was improving; no suppuration. In a third the movements were good as far as a right angle, when the patient left the hospital. In the fourth case the patella was very much broken up by the bullet and portions of the bone afterwards exfoliated. The wound had quite healed in two months. When the patient was discharged about two and a half months after the accident, the patella was freely movable and the joint could be moved by the surgeon to an angle of 150°. The patient did not return to have the passive motion kept up, and when seen a year after the accident, the joint was anchylosed.

I have already alluded to the remarkable results obtained by Dr. Carl Reyher[1] during the recent Russo-Turkish war, and I must now mention in detail these results in cases where joints were injured.

Reyher divides the cases treated into 'Primary antiseptic cases,' 'Secondary antiseptic cases,' and 'Non-antiseptic cases.' By 'Primary antiseptic' cases he means those which were treated aseptically from the very first, and which had not been examined beforehand with dirty fingers or instruments, or treated in any way. In these cases Reyher either washed out the wound with an antiseptic solution, removed any foreign bodies present, drained and treated aseptically, or he did not wash out the wound at all, but simply contented himself with purifying the exterior and applying an antiseptic dressing. In the latter cases he trusted to Esmarch's idea that the bullet would not carry dust into the interior of the wound. The former practice was adopted where the wound was gaping, and where there was a suspicion that portions of clothing, &c. had been carried in with the bullet; the latter practice, where the edges of the skin were lying together, and where it was thought that the bullet had not carried any extraneous matters with it. By 'Secondary antiseptic' cases he means those which had been examined or treated in some way opposed to aseptic principles before coming into his hands. In a few cases suppuration had already commenced. He tried to purify these wounds by washing them out with carbolic lotion, but the attempt was seldom

[1] Volkmann's *Sammlung*, Nos. 142–143, 1878.

successful. They are, therefore, as I have already pointed out, cases treated with antiseptics, not aseptically. The 'Non-antiseptic cases' were treated either with dry dressing, or with some watery or oily application containing an antiseptic.

Reyher demonstrates completely that all the cases which he mentions were perfectly comparable injuries, and that the results must therefore be due to the different ways in which the patients were treated. Every case, with the exception of very severe wounds caused by shell, was treated at the beginning conservatively.

Forty-six cases were treated by 'primary antiseptics,' and of these 6, or 13 per cent., died. With regard to these 46 cases, however, primary resection was performed in 19, and of these 2 died, leaving 27 cases treated conservatively with 4 deaths, or a mortality of 14·8 per cent. The following were the joints which were injured :—

	Total	Healed	Died	Percentage mortality
Shoulder	1	1	0	0
Elbow	2	2	0	—
Hip	1	0	1	100 p. c.
Knee	18	15	3	16·6 ,,
Foot	5	5	0	—
Total .	27	23	4	14·8 ,,

Eighteen of these cases were treated without washing out the wound; in 9 the wound was washed out and drained. In none of the 27 was resection or amputation necessary.

The causes of death in the 4 fatal cases were as follows :— In the injury of the hip-joint acute inflammation and septic suppuration occurred: here it was found that the bullet had carried in a portion of the clothing. One case of knee-joint injury died from fatty embolism within twenty-four hours. One knee-joint case died on the fifth day from hæmorrhage from the divided popliteal artery and vein. The fourth case had not died when the report was issued, but a fatal result was considered certain from hectic fever and diffuse suppuration; I have therefore included it among the fatal cases.

Seventy-eight cases were treated by 'secondary antiseptics,' and of these 48, or 61·5 per cent., died. Of the remaining 30 cases, only 8 ultimately retained their limbs, secondary resec-

tion being necessary in 15 cases, and secondary amputation in 7.

	Total	Lived	Died	Percentage mortality
Shoulder-joint . .	7	4	3	42·8 p. c.
Elbow	11	8	3	27·2 ,,
Hand	5	4	1	20 ,,
Hip	4	0	4	100 ,,
Knee	40	6	34	85 ,,
Ankle	6	4	2	33·3 ,,
Tarsus	5	4	1	20 ,,
Total .	78	30	48	61·5 ,,

The causes of death were in 17 cases pyæmia; in 16, septic inflammations; in 4, very acute suppuration of the joint; in 9, hectic; in 1, carbolic acid poisoning; and in 1 there was jaundice.

Contrasted with these two sets of cases, there were 62 where no antiseptic precautions were taken. Of these 39, or 62·9 per cent., had died when the first report was issued, but in the Appendix we are told that 9 more had died, the mortality being thus raised to 77·4 per cent. The causes of death in the 39 cases were, in 23, from pyæmia; in 6, from septic inflammations; in 6, from hectic; and in 1 the cause was unknown.

The joints involved were (first report):—

	Total	Healed	Under treatment	Died	Percentage mortality
Shoulder .	7	0	4	3	42·8 p.
Elbow .	11	0	5	6	54·5 ,,
Wrist .	6	1	3	2	33·3 ,,
Hip . .	4	0	1	3	75 ,,
Knee .	23	1	4	18	78·2 ,,
Ankle .	11	1	3	7	63·6 ,,
Total .	62	3	20	39	62·9 ,,

The amended table a month or two later would probably be:—

	Total	Healed	Under treatment	Died	Percentage mortality
Shoulder .	7	0	3	4	57·1 p. c.
Elbow .	11	0	5	6	54·5 ,,
Wrist .	6	1	3	2	33·3 ,,
Hip . .	4	0	0	4	100 ,,
Knee .	23	1	0	22	95·6 ,,
Ankle .	11	1	1	9	81·7 ,,
Total .	62	3	12	47	75·8 ,,

One additional case died, but I do not know what joint was injured. The number of limbs treated ultimately conservatively is not mentioned.

A similar result is obtained if similar injuries are compared. Thus take the wounds of the knee-joint; and first the cases in which the bullet became embedded in the bones and in which conservative treatment was tried.

	Treated conservatively to end			Intermediate amputation			Secondary amputation				
	Recovered	Died	Total	Recovered	Died		Recovered	Died	Total	Total	Percentage mortality
Primary antiseptic	4	0	4	—	—	—	—	—	—	4	—
Secondary antiseptic	0	8	8	0	2	2	1	4	5	15	93·3
Non-antiseptic	0	4	4	- -	—	- -	0	5	5	9	100

These facts are surely striking enough. Out of 28 cases of wound of the knee-joint where the bullet became embedded in the bones only 5 lived, and of these four were treated aseptically and retained their limbs. One case recovered which was treated with 'secondary antiseptics,' but only after secondary amputation.

The results are equally striking if we take all the cases of wound of the knee-joint.

	Treated conservatively to the end		Secondary resection		Intermediate amputation		Secondary amputation		Total			Recovered	
	No.	Died.	No.	Died	No.	Died	No.	Died	No.	Died	Percentage mortality	Joints movable	Joints stiff
Primary antiseptic	18	3	—	—	—	—	—	—	18	3	16·6	15	—
Secondary antiseptic	19	18	—	—	9	7	12	9	40	34	85·	—	1
Non-antiseptic	9	6*	1	1	—	—	13	11	23	18	78·2	—	—

* Had died at time of report.

(The last line must be amended, for ultimately only 1 of

these 23 non-antiseptic cases lived. Hence the percentage mortality was 95·6.)

Thus, of 18 aseptic cases 15 recovered, all with movable joints. The causes of death in the 3 aseptic cases are given on p. 449.

Of 63 cases treated otherwise, 7 recovered (5 after amputation, 1 with a stiff knee, and in 1, I think, the knee was movable).

These results are surely convincing. Reyher truly remarks, 'Das scheint mir eben das Bemerkenswerthe zu sein! Nicht dass Knieschüsse conservativ heilten, sondern dass von 18 Knieschüssen welche mir ohne Auswahl auf dem Verbandplatz zugingen und welche an Schwere der Verletzung den andern nicht nachstehen, ein so grosser Procentsatz, nämlich 83·3 pro cent. mit Erhaltung und Beweglichkeit des Gelenkes geheilt ist.'

Bergmann [1] also got some remarkable results by a smilar method of treatment during the same war. When the wound was valvular he did not wash it out at all, but simply purified the exterior and applied an aseptic dressing. 'After the storming of Telisch and Gorni-Dubnik 15 cases of compound fracture of the knee-joint came under treatment, mere injury of the capsule being excluded from the list. Of these 14 recovered, 2 after amputation, and 1 died, also after amputation.' (MacCormac.)

In comparing these results from gunshot injuries with those obtained by other surgeons, MacCormac says: 'Hennen, Larrey, and Guthrie all agree that gunshot wounds of the knee-joint demand amputation, as the result is otherwise fatal. Guthrie states in his book that he cannot recollect a case of recovery after gunshot fracture of the articular ends of the bones. Longmore tells us that in the Crimea not a single man wounded in the knee-joint recovered without amputation.' Such statements at once do away with any objection which might be made to Reyher's cases, to the effect that 18 is a very small number of cases; for among these there were no less than 15 recoveries with movable joints, a result not obtained at all according to Longmore throughout the Crimean war, and not

[1] *Ueber die Behandlung der Schusswunden des Kniegelenks in Kriege.* Stuttgart, 1878.

obtained in the 63 other cases mentioned by Reyher. Indeed, if we were to work out Reyher's tables, we should find that of 600 cases of gunshot wound of the knee-joint treated with antiseptics, but not aseptically, only 15 would retain unmutilated limbs, and the joints would be stiff. On the other hand, we only require to take 18 cases and treat them aseptically in order, not only to retain 15 limbs entire, but also to retain movement in the joints.

The proper proportions would perhaps be got by comparing Reyher's aseptic results with those obtained during the Franco-Prussian war, as published by Heintzel.[1] These cases were treated in a variety of ways; by antiseptics, aseptically, by the water-bath or irrigation, by the open method, &c. Thus the results are by no means so unfavourable as during the Crimean and other wars. The sanitary arrangements were also better. In fact the treatment was much more antiseptic than formerly. Heintzel gives details of 529 cases of gunshot wounds of the knee-joint which were treated at the first conservatively. In 288 instances amputation was afterwards necessary; of these 225, or 78·2 per cent., died; of the 241 in which conservative treatment was carried out to the end, 109, or 45·2 per cent., died. Thus, of 529 gunshot wounds of the knee-joint treated in a variety of ways, generally more or less antiseptically, only 132 or 24·9 per cent. recovered. Compare with this result Reyher's 83·3 per cent. of recoveries where the aseptic principle is thoroughly and logically carried out.

I can find no statistics of the results obtained in similar injuries or operations by other methods of antiseptic treatment. We have seen in Reyher's paper the result of treatment by antiseptics.

Treatment by irrigation, unless a strong antiseptic lotion is employed, is hardly suitable for these injuries, for there are so many opportunities for retention and fermentation of discharge in the interior of a joint. Treatment by water-bath is no doubt sometimes good, but only as *keeping down* the inflammation, not as *preventing* the suppuration. The only method which prevents inflammation and suppuration is the aseptic

[1] *Deutsche militairarztliche Zeitschrift*, 1875.

method, whether it be carried out by operating subcutaneously, or by making a crust, or by the use of antiseptics in the manner recommended by Mr. Lister. The latter is, I believe, the only method by which a wound in a joint may be kept widely open for some days without the occurrence of inflammation or suppuration. In order, however, to obtain this result, it is necessary to observe the minutest precautions before detailed, and to act in the very strictest manner in accordance with the particulate theory of fermentation. It is the omission of this care which leads to the failures which are recorded as failures of the aseptic method, but which are really failures on the part of the surgeon who attempted to carry it out. Surgeons are too apt to regard the omission of details, such as the purification of hands or instruments, as 'trifling oversights,'[1] and it is by so doing that they fail to obtain the results they are led to expect, and that we have such an amount of contradictory evidence before us.

That no precaution is too minute to be attended to, is well illustrated by Mr. Lister's case of removal of loose cartilage from the knee-joint (No. 22, p. 434), which was referred to by him at the meeting of the British Medical Association at Cambridge in 1880. The case was one of loose cartilage in the knee-joint of a strong healthy man which was removed with aseptic precautions by Mr. Lister. The operation was performed on April 12, 1880, and there was no difficulty in its performance. After the cartilage had been removed, a drainage-tube was inserted into the joint, and a small dressing was applied. The same evening the discharge was so profuse that the dressing had to be changed. The discharge had no smell, and I may say that throughout the case it was entirely free from odour.

On April 13, twenty-four hours after the operation, a specimen of the discharge was taken from the drainage-tube, and after staining with methyl violet, it was found to be full of organisms. These organisms were very small, and on careful examination they were found to be of a distinctly oval form and arranged in pairs. In some parts it was very difficult to

[1] See Mr. Holmes's report of a case in which he operated for ununited fracture of the patella in the St. George's Hospital Reports for 1880.

say whether they were round or oval (see Plate V, Fig. 36). There was none of the characteristic grouping of micrococci, no threes in triangles and no pairs side by side. This was seen not to be due to their being all in pairs, for chains of several individuals were found, but nowhere the micrococcal grouping. Again, in a specimen of this pus kept moist on a slide for twenty-four hours, very long chains were found, but no masses or typical micrococcal formation. Hence, from the microscopical examination alone, I concluded that these organisms were not ordinary micrococci, and other evidence will be mentioned immediately which supports this view. They were probably bacteria or micrococci of a peculiar kind not causing *putrefactive* fermentation, but the products of whose growth were very irritating, for that is the only way of accounting for the profuse discharge from the interior of the joint.

Redness occurred around the wound, and spread a little distance up the thigh. In the meantime, the patient's temperature became high and the constitutional disturbance was considerable.

On April 15, there was still profuse discharge, now distinctly purulent, but no smell; and in the stained specimens of the discharge taken on this day, the organisms presented exactly the same characters as on the 13th.

On the 16th, a quantity of pus was pressed out of the joint; an incision was also made into the inflamed part in the thigh and a quantity of sero-purulent matter escaped. A drainage-tube was inserted into this abscess.

On April 18, the state of matters continued the same. In the stained specimens of this day's discharge, numerous organisms were seen, many of which were distinctly oval in form, but a large number were round (Fig. 37, Plate V). Two flasks of cucumber infusion were inoculated from the wound on this day, but no development occurred in them.

In spite of the incision into the thigh, the suppurative inflammation spread, and a large abscess cavity was soon formed. As the drainage of the joint was not good, an opening was made on the outer side and a drainage-tube inserted. There was distinct improvement after this. The acuteness of the process soon began to abate.

On April 22, the discharge from the drainage-tube of the abscess was found to contain organisms of the same characters, though fewer in number than formerly (Fig. 38, Plate V). On this day a flask containing *meat* infusion, and one containing *cucumber* infusion, were inoculated with discharge from this abscess, but *no development* occurred in either.

The state of matters continued with but slight improvement, and organisms were always present, though not so numerous as at first. On April 27, a flask containing *alkalised cucumber* infusion and a fresh *egg* were each inoculated with discharge from the inner side of the knee, but *no development* occurred in either. And on April 30, two eggs were inoculated with discharge from the outer side of the knee without any result.

On May 9, discharge taken from the abscess cavity showed the presence of organisms few in number but possessing the same characters as formerly. A flask of pure *vitreous humour* of a sheep and a flask of ordinary *cucumber infusion* were inoculated from the abscess cavity. *Nothing developed in either* fluid.

With regard to the ultimate result in this case, I may say that improvement now began, and healing was complete and the limb restored to a sound state, during the month of September, though there had been very little indeed to heal for a month or six weeks previously. There was a considerable degree of fibrous anchylosis. The patella was quite movable laterally, and there was a little movement between the tibia and femur, when the patient was discharged.

Now, first as to the nature of these organisms. They were certainly not ordinary micrococci, for they had not the same appearance or mode of growth, and also they did not grow in any of the various materials into which they were introduced. The micrococci of which I have spoken in a former part of this work, and, indeed, any which I have as yet come across, would have grown with great readiness in several of these fluids. This was evidently a peculiar form of organism (probably bacterium), not causing putrefaction, but nevertheless, locally of a very noxious character. Had it not been for the microscopical examination, one might have concluded that no organisms were

present and been puzzled to explain the phenomena on antiseptic principles, on the supposition that organisms were absent.

How did they get in? If the aseptic method is sufficient, as I have asserted it is, how were these organisms admitted? As we have already seen, the only organisms which usually get into aseptic wounds are micrococci. Here the organisms differed in many respects from those ordinarily found, and the ordinary forms of micrococci were absent. Hence they probably did not get in in the same way as micrococci usually do, that is, as I have previously shown, through the dressing; for the ordinary forms of micrococci are the *first* to enter, and would, I think, have been found had that been the explanation. Not only so, but these organisms must have got in at the time of, or very soon after, the operation, in order to be present in such numbers on the next day and also to account for the unusual amount of discharge on the evening of the operation.

And they did not get in through the circulation, for there also micrococci are found as a rule first, and here there was no disturbance of the general health to account for their existence.

Whence, then, did they come? Were they perhaps spontaneously generated? Truly an awkward sort of spontaneous generation for the patient! But why should the mere removal of a loose cartilage from a joint lead to the spontaneous development of organisms in it? There was no formation of a vacuum in the joint. There was no change in the physical forces. There was no introduction of cheese à la Bastian into the wound. If organisms were to develop spontaneously in the joint, why did they not do so the day before the operation rather than immediately after it? I need not pursue the many arguments against this view; for we have previously seen that in exact proportion to the care in making experiments do the facts supposed to favour spontaneous generation disappear.

Two explanations therefore remain. Either we had here to deal with some form of resisting spore, or with one which escaped the action of the carbolic acid, or there was some loophole in the method. The former supposition I am inclined to reject, for various reasons which I cannot discuss here: we

have certainly had no previous experience of bacteria resisting the means employed.

There was, however, I believe, a very distinct loop-hole in the method as practised at that time. This is alluded to at length on p. 79. I had long thought that the drainage-tube might carry into a wound dust-laden air in its interior, if the air which entered the tube on removing it from the carbolic lotion were not purified by carbolic spray. This is the explanation I offer here, viz. that the air carried into the wound by the drainage-tube was not pure air, but contained a particle or particles which gave rise to this particular form of organism. On mentioning this to Mr. Lister, he at once saw the force of the argument, and since that time he takes precautions to prevent the recurrence of a similar accident.

It is just possible that the fault lay in the fact that a very small dressing was applied at the time of the operation, and that they got in through the dressing. But then they must have got in during or very shortly after the operation, in order to produce the rapid effect which followed. And also, as I have just stated, the ordinary forms of micrococci would probably have been found in that case.

The only other explanation would be, that there was some error in the manipulations. But though, no doubt, faulty manipulation is almost the constant cause of failure in those commencing the practice of this method and in those who have practised it, when the operation is difficult and requires much thought, yet here there was a simple operation, the importance of the aseptic precautions were fully present to the mind, and I do not think that, especially in the case of Mr. Lister and his assistants, this explanation is in this instance the most probable one. Whichever explanation, however, be correct, the case is of great importance, as confirming the views expressed before, as illustrating the necessity of excluding organisms of all kinds, and as showing the results which may follow the admission of bacteria to wounds. It is also of interest, because it shows that forms of bacteria may be present without the production of smell, and thus, because a discharge is '*sweet*,' it is not necessarily '*aseptic*.' The difficulty which was experienced in finding the organisms before the pus was

stained also warns us how careful we must be in deciding whether organisms are present or absent from wounds. Cases have been published where disastrous consequences have followed incisions into joints, but where the surgeon has asserted that the discharge was sweet and free from organisms. According to recent investigations, however (p. 253), we know that the pus of acute abscesses and from acute suppurations always contains organisms, generally micrococci; and therefore, in the published cases in which the assertion to which I have alluded has been made, it is clear that the presence of organisms has been overlooked; for if no other form was present, micrococci were sure to be there.

Various facts are known as to the removal of foreign bodies from joints without aseptic precautions. A number of these have been referred to in the historical part, and I will here content myself with quoting some figures from a paper published by M. Larrey in 1861. As we have seen in considering the History of Antiseptic Surgery, the valvular method introduced a considerable amount of security into these operations. Larrey groups the results of the direct and of the oblique, or valvular incision together, but the details are very meagre. He mentions 132 cases, of which 30 died; 7 were unsuccessful or uncertain in result; in 10 he mentions the occurrence of anchylosis; in 2 of hydrarthrosis; and in 9 of grave accidents not further specified. There are thus left 74 cases which were possibly cured with movable joints, but he expressly states that all the cases of healing in which accidents occurred are not noted, so that the number of cures is really less. Thus, only about one half of the cases recovered without the occurrence of some serious complication, yet these results, though apparently so bad, are much more favourable than those which had been obtained by several other surgeons. Larrey contrasts with these cases the results of the operation in which a bed is made outside the joint to receive the cartilage. Of these he mentions 38 cases with 19 cures, 15 failures to extract the cartilage, and 5 deaths.

Larrey's statistics express very well the opinions of the majority of surgeons on the dangers of extracting foreign bodies from joints, even when the valvular or subcutaneous

method is employed. If we search the surgical text-books we find that there is throughout a fear of such operations or injuries. Thus, Mr. Spence, in his 'Lectures on Surgery,' says,[1] 'There is no class of operations that I have a greater dread of than the apparently simple one of removing a loose cartilage.'

I have no statistics of septic cases, comparable to the aseptic ones, in which wounds of joints have been kept open for some days; but, as will be seen in the history of this subject, experience has up till recently led surgeons to the conclusion that the safety of the limb and of the patient depends on rapid healing of the wound. And yet, as all the facts quoted show, these operations, when aseptically performed, are really devoid of danger. Sir James Paget says:[2] 'I cannot doubt that operations of this kind' (referring to incisions of joints with closure of the wound), 'which, in the earlier years of my work, were done with great risk, or, with a wise fear of the risk, were left undone, may now, with antiseptic help, be done with an almost complete safety.'

[1] See leading article in *British Medical Journal*, April 1880.

[2] MacCormac's *Antiseptic Surgery*.

CHAPTER XIX.

RESULTS OF ANTISEPTIC SURGERY (*continued*).

Compound fractures. Differences between those produced accidentally and those caused by the surgeon: treatment and after-progress of each class. Tables of accidental compound fractures treated by Mr. Lister: *thigh; leg; humerus; forearm; skull; summary of results.* Tables of compound fractures produced intentionally by Mr. Lister; *femur; leg; clavicle; humerus; forearm; lower jaw.* General summary of Mr. Lister's results. Mr. Spence's results. Other operations on bones by Mr. Lister. MacEwen's osteotomies: Volkmann: Max Schede: Bardenheuer: MacCormac. Combined aseptic results. Results by other methods: Volkmann and Fraenckel: Holmes: St. Thomas's Hospital. Reyher's results in war. Open method: Krönlein. Septic methods.

I NOW pass on to a second class of cases which are often followed by most serious consequences: I refer to compound fractures occurring accidentally or made by the surgeon.

Compound fractures produced accidentally and those made by the surgeon differ from each other in various important particulars. In the first class dust is as a rule introduced into the wound before the surgeon sees the case, and, therefore, the problem is to destroy the energy of this dust. Whether such an attempt is successful or not, must of course always be a matter of doubt; and hence the results are uncertain. Then, also, the violence is often very severe and complicated with other injuries or with shock, and in this way life may be lost from causes which could not be avoided by any method of wound treatment. On the other hand, in the second class of cases the surgeon has only the ordinary aseptic problem before him, and if he is justified in other cases in looking with certainty for good results, he ought to be equally justified here. He is also independent of the other injuries and shock which so often complicate accidental compound fractures, and, therefore, the mortality ought also to be less. If we remember

these differences, we shall be able to form a just estimate of the value of the results.

The aseptic course in these cases corresponds to that described in Chapter XVIII.; and I need only indicate one or two points in the treatment and after-progress of the injury.

The treatment of compound fractures the result of accident has been already described at pp. 113 and 114. It consists in washing out the wounds thoroughly with a strong antiseptic lotion, generally the 1–20 watery solution of carbolic acid or the 1–5 mixture of carbolic acid and methylated spirit. This is done by injecting the lotion through a catheter attached to a syringe filled with the solution, the orifice of the wound being left freely open. In this way, by moving the point of the syringe in various directions, the lotion is introduced with certainty into all the recesses of the wound, while by leaving the orifice of the wound freely open there is no risk of forcing the fluid into the cellular tissue. It is well to clear out all the clots of blood. The skin in the neighbourhood is also thoroughly washed with 1–20 carbolic lotion, the whole being done in a spray of carbolic acid. The wound is left freely open, and in most cases a drainage-tube is passed into the deeper parts and kept in for a few days. In some instances, if there is much tendency to displacement, the ends of the fragments may be tied together with strong silver wire. A large gauze dressing, enveloping the limb, is then applied, and outside it a suitable splint. This dressing is changed on the following day, and afterwards according as it is necessary. After a few days, in compound fractures of the lower extremity, when the discharge has become small in amount, some arrangement like that described at p. 107 may be employed.

The after-progress of these cases depends on whether the causes of putrefaction were destroyed or not by washing out the wound. If they were not, then the case becomes one treated with antiseptics, but not aseptically. If the causes of putrefaction were eradicated, the wound follows an 'aseptic course' (p. 421). In cases of compound fracture, more especially from direct violence, the soft parts are often much contused and lacerated, and the bones are sometimes comminuted and much injured. The ordinary result in such cases, when

aseptic treatment is not employed, is, that sloughing of the contused and lacerated tissues occurs to a greater or less extent, and very generally portions of the broken fragments of the bone become necrosed. This process is accompanied with a considerable amount of suppuration. If, however, the wound is rendered aseptic, and if the irritation of the antiseptic is excluded, this sloughing and suppuration does not occur. The wound becomes filled with blood-clot; the interstices between the fragments of lacerated tissue also become filled up; the whole remains unaltered for many days, merely assuming a greyish appearance on the surface, but after some days, on scratching this clot, it bleeds, showing that it has become vascularised, and on detaching the superficial layer the greater part, or

XXII

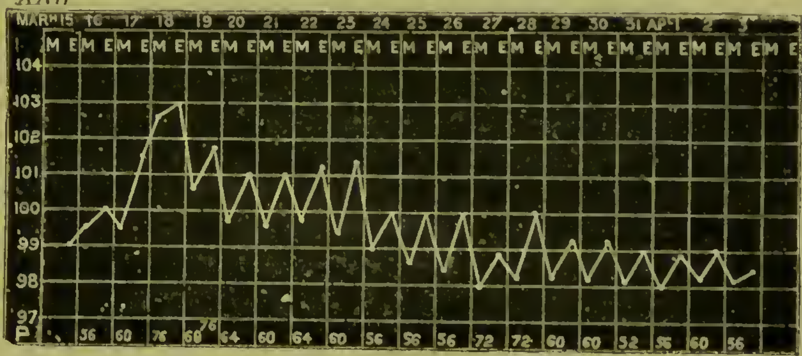


FIG. 78.—TEMPERATURE CHART FROM A CASE OF COMPOUND FRACTURE, IN WHICH THE ATTEMPT TO ERADICATE THE CAUSES OF FERMENTATION WAS UNSUCCESSFUL, AND WHICH THEREFORE BECAME A SEPTIC CASE (CASE 26, p. 472).

indeed the whole of the wound, will be found to have become covered with epithelium. (This process will be more minutely discussed hereafter.) There is no suppuration at all from the wound, and no separation either of dead skin or tissue, or of dead bone. If, however, no protective be used, or the wound be deluged with strong carbolic acid, the superficial layer of the clot becomes irritated, and when vascularised, granulates, and suppuration occurs from its surface; at the same time, the portions of dead tissue being impregnated with carbolic acid, become irritating, cause granulation and suppuration where they are in contact with the living parts, and are thus separated as sloughs. The sloughing in this instance, however, is not as a rule nearly so extensive as in the case of a septic wound.

The constitutional condition also depends in the main on the success or failure of the attempt to render the wound aseptic. If the attempt fails, the temperature is generally high, as in other septic cases (see fig. 78); if, on the other hand, it is successful, the temperature generally remains normal or nearly so, though it may be high for a few hours after the injury (see fig. 79). (I shall not go into this matter further at present, as I intend to discuss it more fully in a future chapter.) The general well-being of the patient also closely corresponds to the septic or aseptic state of the wounds; if the wound is septic and the temperature high, the patient generally feels ill, and has other symptoms of fever; if, however, the wound is

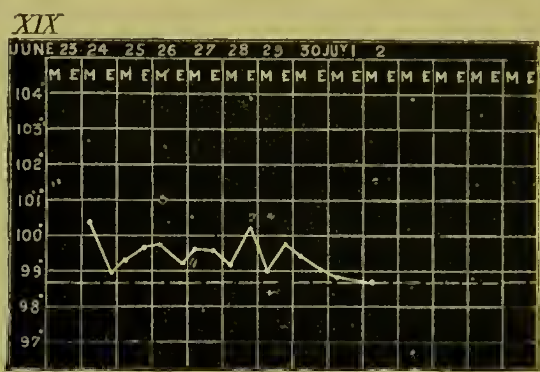


FIG. 79.—TEMPERATURE CHART FROM A CASE OF COMPOUND FRACTURE WHICH FOLLOWED AN ASEPTIC COURSE (CASE 23, p. 472).

rendered aseptic the patient feels practically in a normal state of health.

In the case of compound fractures made by the surgeon, the progress of the wound and the constitutional state of the patient are similar to those described as typical of operations on joints (p. 421); and I need not repeat what I said there. In performing these operations a free incision is made down to the bone with the various aseptic precautions; the bone is chiselled or sawn across, or a portion of bone is removed according to circumstances; the bleeding vessels are secured; a drainage-tube is introduced down to the bone; as a rule, no stitches are inserted; and the dressing is applied according to the ordinary rules previously described. In a few hours the wound is filled with blood-clot, which becomes vascularised and organ-

ised, and cicatrisation occurs beneath its superficial layer without previous granulation or suppuration. As a proof of this I may refer to the fact mentioned by Dr. MacEwen, that he had made 835 compound fractures, and that in only 8 of the wounds was there any pus formation, although none of these injuries was subcutaneous. After operations conducted in this way the general state of the patient remains normal.

In considering the results of compound fractures, it is quite clear that all cases, in whatever way they are treated, whether by primary or secondary amputation or conservatively, must be mentioned and grouped together in the first instance. For the frequency of primary amputation after compound fracture will depend not only on the great severity of the injury and destruction of tissue, but also on the dangers to which, in accordance with the experience of the surgeon in charge, the patient will be afterwards subjected. If the surgeon feels that the method of treatment which he has been accustomed to adopt in these cases is not such as to secure the patient from the after consequences, he will naturally take into frequent consideration the question of primary amputation. If, on the other hand, he has been led to expect that the dangers incident to these injuries are not likely to occur under the method of treatment which he adopts, he will perform primary amputation less frequently. Therefore, in order to judge of the results of any method of treatment in compound fractures, the number of primary amputations and the sort of injuries in which they are performed must be mentioned. With regard to secondary amputation it must be remembered that these, as well as the fatal cases, are those in which the attempt to treat conservatively has failed, and therefore it is not sufficient merely to state the results of cases treated conservatively to the end. In the following tables I have attempted to indicate, as fairly as possible, the nature of the injuries and the after-progress of the cases which have occurred in Mr. Lister's practice since 1872. In some instances the notes have been deficient, but I have tried to render them complete by tracing out these cases as far as I could.

I. Compound Fractures, the

In considering these I have included *all* Mr. Lister's cases since 1871: those requiring primary amputation as well as those treated

Compound Fractur

No.	Name and Age	Date of Admission and Discharge; with Result.	Injury
1	A case of extensive compound fracture of the femur where primary amputation wa performed at the hip-joint. The patient died almost immediately. There is no record		
2	J. S., 45 . . .	*Ad.*, June 26, 1874. *Died.*, June 26, ,, Cause of death was shock.	Extensive compound comminuted fracture of the femur, and severe laceration of leg
3	George P., 12 .	*Ad.*, Sept. 29, 1871. *Dis.*, Feb. 14, 1872. In process of cure.	Compound fracture of the femur from a fall. Direct violence.
4	R. P., 17 . . . (Septic case)	*Ad.*, Nov. 15, 1872. *Died*, Dec. 14, ,, Cause of death was bronchitis and cardiac disease.	Two railway trucks passed over his leg causing a simple comminuted fracture o both bones of the leg, and a simple commi nuted fracture of the thigh. The skin over the fracture of the femur sloughed and the bone protruded on November 22 Patient was suffering from cardiac disease
5	Mrs. D., 50 .	*Ad.*, Oct. 15, 1878. *Dis.*, June 20, 1879. *Result*, healed.	Compound fracture of the femur.

There were thus five compound fractures of the femur, of whi two were amputated primarily, both dying of shock; and three we treated conservatively, one of these dying of bronchitis and cardi

Compound Fractu

6	William R., 12	*Ad.*, June 17, 1872. *Died*, same day. Cause of death was shock.	Compound fracture of left leg and right thig Skin torn off for a considerable distanc Patient in a state of collapse when a mitted.
7	A. R., 18. . .	*Ad.*, April 7, 1874. *Dis.*, Sept. 3, 1874. *Result*, cured.	Leg crushed by a beam. Severe compoun comminuted fracture of the leg.

ult of Accident.

servatively or amputated secondarily. I omit the lesser compound tures of the hand and foot.

the Femur (*accidental*).

Treatment	Remarks
ase in the books, but I know that it occurred.	
nary amputation just below the ochanters.	Patient was unconscious when admitted, and never rallied. He died in about two hours. Cause of death was shock.
and injected with 1-20 carbolic lotion. pening enlarged. Drainage-tube inrted, and splints applied.	The bones had firmly united on November 14, and the patient seems to have done well. On January 19 the sinus had not yet closed, and Mr. Lister introduced a pair of sinus forceps to see if there was any loose bone. No precautions were used to disinfect the air which entered (no spray or carbolic lotion), and putrefaction seems to have followed. When the patient was discharged there was still a piece of bare bone to be felt, but he was in perfect health.
attempt was made to keep the slough rect, but putrefaction had already curred, and therefore the attempt as unsuccessful.	Abscesses formed about the knee and higher up in the thigh. The notes are incomplete, but the patient is entered in the hospital books as having died of bronchitis and cardiac disease, from which he was suffering previous to admission. (This was a septic, not an aseptic, case, so that whatever was the cause of death, it does not influence the result in aseptic cases.)
No. 9, p. 424, and No. 70.	Aseptic course. The wound healed, but the bones did not unite. (See T. Chart 11, p. 438.)

ase. This last was, however, a septic case, and therefore all the (two in number) of compound fracture of the femur treated ervatively and aseptically recovered.

f Leg (*accidental*).

Treatment	Remarks
Pary amputation through the upper left leg and upper ½ of right thigh.	Patient never rallied, but died in two or three hours from shock.
Pary amputation. (Modified Carden.)	For some time the patient suffered from carbolic poisoning, but after the carbolic dressings were left off and boracic dressings were substituted for them, he soon recovered, and the stump healed slowly.

I. Compound Fractures, Ti

No.	Name and Age	Date of Admission and Discharge; with Result	Injury
8	F. D., 22 . . .	*Ad.*, Dec. 12, 1873. *Dis.*, March 13, 1874. *Result*, cured.	Railway engine passed over leg, almost sever ing the foot, and producing a severe com pound comminuted fracture of the leg.
9	James B., 18 .	*Ad.*, Aug. 3, 1874. *Dis.*, Sept. 13, ,, *Result*, cured.	Railway engine passed over his ankle, cru ing the bones very severely. A conside able amount of shock.
10	R. H., 25. . .	*Ad.*, March 17, 1874. *Dis.*, April 27, ,, *Result*, cured.	Very severe compound fracture of both bon of the leg. Foot drawn through a por hole of a vessel by an anchor chain.
11	George A., 41 .	*Ad.*, July 1, 1872. *Died*, July 3, ,, Cause of death was exhaustion.	Compound comminuted fracture of b bones of the leg.
12	George S., 24 . (Septic case)	*Ad.*, March 15, 1872. *Died*, May 20, ,, Cause of death was diphtheria.	Compound comminuted fracture of b bones of the left leg. Fracture of ri thigh with very severe bruising. Pati almost moribund.
13	James D., 24 .	*Ad.*, Aug. 2, 1872. *Dis.*, Sept. 17, ,, *Result*, healed.	The wheel of a cab passed over the l causing a compound fracture. Sn wound.
14	Peter M., 22 (Septic case)	*Ad.*, Sept. 9, 1872. *Dis.*, Oct. 31, ,, *Result*, cured.	Severe compound comminuted fractur both bones of the leg. Leg crushed l heavy stone.
15	Anne L., 60 .	*Ad.*, Oct. 28, 1872. *Died*, on same day. Cause of death was shock.	Compound comminuted fracture of the Also fracture of the pelvis. Patient m bund when admitted.

ULT OF ACCIDENT (*continued*).

Treatment	Remarks
mary amputation through the calf.	Hæmorrhage occurred on several occasions in connection with a portion of the anterior flap which sloughed, but the rest of the wound did well, and the whole was quite superficial on February 1, and quite healed when the patient was discharged.
nary amputation through the upper hird of the leg. (Modified circular.)	Aseptic course (*i.e.* no local or constitutional disturbance). The wound healed by first intention except where the drainage-tubes were and at the centre of the flaps, where a little gaping occurred. The wound was absolutely healed on September 18.
nary amputation just below the knee. Modified circular.)	The line of incision healed for the most part by first intention. A slight tendency to gaping at the centre of the incision was easily overcome by the use of strapping applied aseptically. Wound was quite healed when the patient was discharged.
und enlarged and injected with 1-20 rbolic lotion. Wound left open.	Patient got gradually weaker, and, without any special symptoms, died in forty hours.
unds washed out with 1-20 carbolic ion. (Secondary amputation.)	Putrefaction was not avoided, and on March 17, when this was evident, amputation was performed through the middle of the thigh. This wound had almost completely healed when, on May 17, he complained of sore throat. His temperature went up; diphtheritic patches appeared on the fauces; and he died on May 20 from diphtheria. (See T. Chart XVIII.)
und enlarged and injected with 1-20 rbolic lotion. Wound left open.	Aseptic course. The temperature only once rose above 100° F., and then it reached 101° F., thirty-six hours after the accident. The wound had quite healed, without any suppuration, by September 7. The bones were not quite firm when the patient was discharged. He was sent out with the leg in a case of plaster of Paris.
ed out with 1-20 carbolic lotion. econdary amputation.)	Putrefaction was not avoided, and as the temperature was going up (it had reached 103°), Mr. Lister thought it better to amputate. This he did through the lower third of the femur on September 13. The stump followed a perfectly aseptic course, and healed by first intention, except where the drainage tubes were. It had completely healed on October 18.
nd injected with 1-20 carbolic lotion.	Patient died three hours after admission. On p. m. examination extensive fractures of the sacrum and pelvis were found.

1. Compound Fractures, th

No.	Name and Age	Date of Admission and Discharge; with Result	Injury
16	William K., 16	*Ad.*, March 26, 1873. *Dis.*, July 12, ,, *Result*, cured.	Compound fracture of both bones of the leg. A piece of stone fell on his leg. Admitted twenty-four hours after the accident.
17	J. M., 32. . . (Septic case)	*Ad.*, June 16, 1873. *Dis.*, Sept. 15, ,, *Result*, cured.	The wheel of a tramway car passed over h leg, causing a compound comminuted frac ture of both bones. Patient admitted im mediately.
18	J. McA. . . .	*Ad.*, Aug. 22, 1873. *Dis.*, Dec. 18, ,, In process of cure.	A stone fell on his leg, causing a compoun comminuted fracture of both bones. Th patient was admitted two hours after th accident.
19	James B., 56 . (Septic case)	*Ad.*, Sept. 15, 1873. *Dis.*, March 12, 1874. In process of cure.	A van ran over his leg, causing compoun fracture of both bones, two inches abo the ankle-joint. Seen at once.
20	A. R., 25 . . . (Septic case)	*Ad.*, Feb. 18, 1875. *Dis.*, Sept., ,, *Result*, cured.	Leg was run over. Severe compound comm nuted fracture of both bones. Admit two hours after the accident.
21	T. N., 30 . . .	*Ad.*, April 26, 1875. *Dis.*, Sept. 20, ,, *Result*, cured.	Compound fracture of tibia caused by wooden beam falling on his leg. Up end of lower fragment protruding. Ac dent happened four hours before admissi
22	Jane L., 36 . .	*Ad.*, Oct. 18, 1875. *Dis.*, March 11, 1876. In process of cure.	Compound fracture of tibia. Leg crushed a wooden beam. Lower end of upper fr ment protruding. Done four hours bef admission.

ESULT OF ACCIDENT (*continued*).

Treatment	Remarks
Vound injected with 1-5 solution of carbolic acid in methylated spirit. Wounds enlarged. McIntyre splint applied.	Putrefaction avoided. A small abscess formed in the leg and was opened on March 31. Afterwards the wounds progressed well, and were superficial on May 16. Boracic dressing was then applied. Erysipelas attacked the wound on May 23, but it had passed off on May 30, and the wounds healed rapidly. The bones were quite firm and there were only two superficial spots to heal when the patient was discharged.
Vound injected with a solution of carbolic acid in methylated spirit, 1-5.	Putrefaction was not avoided. The wound suppurated freely, and a small piece of bone necrosed. The temperature was not regularly taken, but for twelve days it was above 100° F., ranging from 100° to 103·9°. The bones had united and the wounds had almost healed when the patient was discharged.
ome fragments of bone were removed. The wound was washed out with carbolic lotion, 1-20.	Putrefaction was avoided. There was a very little superficial 'antiseptic suppuration.' There was no inflammation or formation of abscesses. When discharged the leg was firm, but there was still a sinus leading down to bare bone. The temperature only once rose to 100°, thirty-six hours after the accident.
Vound injected with 1-20 carbolic lotion.	Putrefaction was at first avoided, but the patient, during an attack of delirium tremens, tore off the dressings, and the wound putrefied. It was henceforth treated with lint soaked in carbolic acid and glycerine. When discharged there was still a sinus leading down to bare bone, but the bones were quite firm.
Vound injected with 1-20 carbolic lotion.	Putrefaction was not avoided. Two abscesses formed, and the wound and the abscess cavities suppurated. For six weeks the temperature varied from 100° to 103° F. Other abscesses formed, but ultimately the parts began to recover, and healing was complete at the end of September.
Vound injected with 1-20 carbolic lotion.	Aseptic course. The highest temperature recorded was 99·7°. The wound had healed (exact date not given), and the bones had united when the patient was discharged.
Vound washed out with 1-20 carbolic lotion.	No constitutional disturbance, but two small fragments lost their vitality, and were not removed till a few days before the patient was discharged. At that time the bones were firm, and there remained only a small superficial sore.

I. Compound Fractures, the

No.	Name and Age	Date of Admission and Discharge; with Result	Injury
23	James G., 11 .	*Ad.*, June 23, 1876. *Dis.*, Nov. 23, ,, In process of cure.	Compound fracture of both bones of the leg. Admitted immediately after the accident.
24	William R., 50.	*Ad.*, Jan. 26, 1877. *Dis.*, March 15, 1877. *Resu t*, cured.	Compound fracture of tibia, caused by a kick from a hor-e. Admitted immediately.
25	Mary V., 55 .	*Ad.*, Nov. 9, 1877. *Dis.*, Aug. 14, 1878. *Result*, cured.	Run over by a cart which caused a compound comminuted fracture of both bones of the leg.
26	Andrew F., 38. (Septic case)	*Ad.*, March 15, 1878. *Dis.*, Dec. 2, ,, *Result*, cured.	The wheel of a van passed over his leg, producing a very bad compound fracture of both bones just above the ankle-joint. Great injury to the soft parts. There was also a compound Colles' fracture. See No. 42.
27	Cornelius N., 34	*Ad.*, May 24, 1878. *Dis.*, Dec. 18, ,, *Result*, cured.	Leg jammed between a stone and the wheel of a truck, resulting in compound comminuted fracture of both bones of the leg.
28	Maria L., 60 .	*Result*, cured.	Compound fracture of fibula, etc. (See injuries of joints, No. 10, p. 424.)

Result of Accident (*continued*).

Treatment	Remarks
Wound injected with 1-20 carbolic lotion.	Aseptic course. The bone took a long time to unite, and was not absolutely firm, though almost so, when the patient was discharged wearing an immovable apparatus. (See T. Chart XIX.)
Wound injected with 1-20 carbolic lotion. Wound left open as usual.	Aseptic course. Beautiful example of organisation of blood-clot, without suppuration. Healing complete on February 28. The tibia was found to be firm on March 6. (See T. Chart XX.)
Wound syringed out with 1-20 carbolic lotion.	Apparently no constitutional disturbance, though the temperature was above 100° on several occasions. A portion of the skin sloughed. No exfoliation. Put up in plaster of Paris on January 23, 1878. Taken down on March 14, but the bones were not then quite firm. One or two apparatuses were subsequently applied, and when the patient was at length discharged, union was complete. (See T. Chart XXI.)
Wound washed out with 1-20 carbolic lotion. On the following day, in order to get the bones in position, it was necessary to saw off portions and tie the fragments together with silver wire.	The attempt to eradicate the causes of putrefaction was not successful, and the wound suppurated. The patient had fever. An abscess formed in the leg. The wounds were quite healed on June 5. The wire was removed on July 23. In November the bones were found to be soundly united. (See T. Chart XXII.)
Wound enlarged and pieces of bone removed. Wound washed out with 1-20 carbolic lotion.	The greatest difficulty was experienced in keeping the bones in position, and there was consequently great pain. The temperature was high for some days, but the pulse was not fast, and the patient seemed well with this one exception. For some days the bones became constantly displaced, but otherwise the wound did well. It remained aseptic, though as the result of the disturbance there was suppuration, but only a very little. As the wound got smaller it was found that the bones could be kept in position by a screw pressing on the upper fragment. The leg was put up in plaster of Paris, but on November 29 the bones were found to be still ununited. Eight to ten minims of tincture of iodine were therefore injected between the fragments. Some swelling and a small abscess resulted, and the bones united. When seen on January 30, 1879, union was complete. (See T. Chart XXIII.)
— —	Patient did well.

I.—COMPOUND FRACTURES, THE

No.	Name and Age	Date of Admission and Discharge; with Result	Injury
29	F. C., 6 . . . (Septic case)	*Ad.*, Dec. 4, 1878. *Dis.*, April 20, 1879. *Result*, cured.	Leg run over by a cab. Large wounds on both sides of the leg, that on the outer side communicating with a fractured fibula. On the inner side the tibia was bare.
30	Alfred A., 6 .	*Ad.*, Sept. 12, 1879. *Dis.*, Oct. 19, ,, *Result*, cured.	Compound fracture of both bones of the leg. Tibia protruding. Cause not given.
31	Frederic H., 48	*Ad.*, Nov. 14, 1879. *Dis.*, Jan. 5, 1880. *Result*, in process of cure.	Compound fracture of the lower end of the right fibula and simple fracture of the middle of the right tibia. Result of a fall.

There were thus under Mr. Lister's care during these years 26 cases of compound fracture of the bones of the leg, of which 4 died. Three of these deaths occurred within forty-eight hours, and the other was from diphtheria. None of the deaths was therefore preventible by any method of after-treatment of the wound.

In 5 of the 26 cases primary amputation was necessary. One of these cases died of shock.

Twenty-one cases were treated conservatively, and of these 3 died.

Of these 21 cases 2 required secondary amputation, of which 1 died of diphtheria. Of the remaining 19 cases 2 died, both from shock and exhaustion, within forty-eight hours.

Compound Fractures of

32	R. J., 23 . . .	*Ad.*, March 22, 1872. *Died*, March 23, ,, The cause of death was shock.	The wheel of a railway waggon passed over his shoulder, causing a compound comminuted fracture of the humerus and scapula.
33	George G., 60 .	*Result*, cured.	Compound comminuted fracture of the humerus.
34	Henry A., 12 .	*Ad.*, Dec. 28, 1872. *Dis.*, April 17, 1873. *Result*, cured.	Cab wheel passed over arm, causing a compound comminuted fracture of the humerus. Patient brought to the hospital immediately.
35	Henry B., 22 .	*Result*, cured.	Compound fracture of the humerus.

Result of Accident (*continued*).

Treatment	Remarks
Wound washed out with 1-20 carbolic lotion.	Putrefaction was not avoided. Sloughing occurred to a considerable extent and the discharge had a foul smell. When this was evident, boracic ointment and boracic lint were used. The temperature chart cannot be found, but for some days the temperature varied from 100° to 103° F. On March 23, two pieces of dead bone were removed. When discharged the wound had healed and the fracture united.
Wound washed out with 1-20 carbolic lotion.	Aseptic course.
Wound enlarged and syringed out with 1-20 carbolic lotion.	Aseptic course. Wound healed about December 20. Fractures found united on December 27. Sent out with a silicate apparatus on.

In these 19 cases the attempt to eradicate the causes of putrefaction was unsuccessful in 4 instances. It was also unsuccessful in both the cases in which secondary amputation was necessary. Thus the attempt failed in 28·5 per cent. of the cases. One case putrefied after some days because the patient tore off the dressings.

In 2 of the 6 septic cases secondary amputation was necessary; necrosis occurred in 1, sloughing of the skin took place in 1, and in the remaining 2 there was inflammation and abscess formation.

In none of the aseptic cases was any operation necessary. In only 1 was there any appearance of necrosis, and the course of the others is markedly different in all respects from that of the cases where putrefaction was not avoided.

the Humerus (*accidental*).

Arm removed and also loose portions of the scapula.	The patient was suffering severely from shock when he was admitted, and he never rallied. Died from shock.
See Wounds of Joints (No. 2).	
Wound washed out with 1-20 carbolic lotion.	Putrefaction was avoided, but late in the case a little pus escaped from the wound. On probing it dead bone was felt. This was removed in June, and the wound healed rapidly. The bones were firmly united on April 11.
See Wounds of Joints, p. 424 (No. 11).	(See T. Chart III., p. 438.)

I.—Compound Fractures, the

Four compound fractures of the humerus were under treatment with 1 death from shock.

Of these 1 case was amputated primarily and died, while 3 cases

Compound Fractures of

No.	Name and Age	Date of Admission and Discharge ; with Result	Injury
36	C. B., 40 . . .	*Ad.*, June 2, 1874. *Dis.*, July 6, ,, *Result*, cured.	A weight of 30 cwt. fell from a height on to his forearm, completely crushing it.
37	J. H., 16 . . .	*Ad.*, June 8, 1877. *Dis.*, July 16, ,, *Result*, cured.	Patient's forearm was drawn into a crushing machine and very badly smashed.
38	Thomas W., 17	*Ad.*, July 19, 1872. *Dis.*, Sept. 22, ,, *Result*, healed.	Hand caught in a printing-machine, causing a compound comminuted fracture of the humerus. Radial artery torn across. Fingers cold and pulseless. Extensive injury of the soft parts. Admitted at once.
39	John S., 34 . .	*Ad.*, July 7, 1873. *Dis.*, Aug. 14, ,, *Result*, cured.	Forearm run over by a vehicle. There was a small wound communicating with a fracture of both bones. The accident happened an hour and a half before admission.
40	J. F., 30 . . . (Septic case.)	*Ad.*, Oct. 29, 1875. *Dis.*, Jan. 31, 1876. *Result*, in process of cure.	Arm caught in machinery. Compound fracture of both bones of the forearm. Radius much comminuted. Soft parts much torn.
41	John O'H., 28 .	*Ad.*, Dec. 24, 1875. *Dis.*, Feb. 11, 1876 *Result*, in process of cure.	Compound fracture of the forearm from a piece of wood falling on it. Seen after two or three hours.
42	Andrew F., 38 .	*Ad.*, March 5, 1878. *Result*, cured. (See No. 26, p. 472.)	Patient had a compound Colles's fracture as well as the fracture of the leg.

There were thus 7 compound fractures of the bones of the forearm without a death. Of these, 2 cases required primary amputation. In one of the 5 cases treated conservatively the attempt to eradicate the cause of putrefaction was unsuccessful. In 2 of the 5 cases union occurred satisfactorily ; in 2 union was delayed, and in one it did not occur on account of the great loss of bone.

If we sum up the results of compound fractures of the extremi-

RESULT OF ACCIDENT (*continued*).

were treated conservatively with good result. The attempt to eradicate putrefaction was successful in each of the 3 cases.

the Forearm (*accidental*).

Treatment	Remarks
Primary amputation through the lower part of the humerus.	Aseptic course. A minute superficial sore remained to heal, where the drainage-tubes were, when the patient was discharged.
Primary amputation above the middle of the humerus.	Aseptic course. Wound healed by first intention, except where the drainage-tube was. Here a small superficial sore remained to heal when the patient was discharged.
Wounds washed out with 1-20 carbolic lotion. Some loose fragments of the broken radius were removed. The wounds were also mopped out with chloride of zinc. Splints applied.	The fingers regained their warmth during the first twenty-four hours. Putrefaction was avoided, and the case went on well. So much of the radius was lost that union could not take place. The wounds were quite superficial on August 19, and had healed when the patient was discharged. (See No. 105.)
Wound enlarged and a solution of carbolic acid in spirit injected.	Aseptic course. Highest temperature 99·9, pulse 82. Date of healing is not given, but healing was complete and the bones were strong before the patient left the hospital.
Wound washed out with 1-20 carbolic lotion.	Putrefaction was apparently not avoided. The wound did very well, but when the patient was discharged it had not quite healed. Bones pretty firm. Patient discharged, wearing an immovable apparatus. (See T. Chart XXIV.)
Wound enlarged and injected with 1-20 carbolic lotion.	Typical course. When discharged to be treated as an out-patient, there was only a small point to heal, and the bones were getting firm. (See T. Chart XXV.)
Wound washed out with 1-20 carbolic lotion.	Followed a typical aseptic course. The arm had healed and the splints were left off on April 12.

ties produced by accident and treated by Mr. Lister, we get the following facts:—

Since 1871 there have been 41 patients (42 limbs) admitted into hospital suffering from compound fractures of the long bones; of these 8 died. In 6 the cause of death was shock and exhaustion, and death occurred within forty-eight hours after the accident; in 1 the cause was diphtheria, and in 1 bronchitis and cardiac disease.

I.—COMPOUND FRACTURES, THE

Of the 42 limbs injured 10 (10 patients) were amputated primarily, and of these four died (all from shock). I need not refer to these again, as they do not concern the point at issue. There were thus 31 patients (32 compound fractures) in which conservative treatment was attempted, and of these 4 died (2 of shock, 1 of diphtheria, and 1 of bronchitis and cardiac disease); but then only 1 of these deaths (case 4, which died of bronchitis and cardiac disease) can possibly be supposed to have any connection with the method of treatment of the wound. Hence, for our present purpose, we have to consider 28 cases (29 fractures) treated conservatively with one death. The fatal result in this instance is stated to have been cardiac disease and bronchitis, but the precise cause does not tell against the aseptic results, for in this case putrefaction took place.

Of the 32 limbs treated conservatively, only 2 required secondary amputation, and one of these patients died of diphtheria, or we may perhaps put it better, and say that of the 29 limbs (28 patients) under consideration, only one required secondary amputation, and this case recovered. One of the remaining 27 patients died of bronchitis and cardiac disease.

Of the 30 limbs treated conservatively in patients who did not die within forty-eight hours, the attempt to eradicate putrefaction

Compound Fractures

No.	Name and Age	Date of Admission and Discharge; with Result	Injury
43	William S., 18.	*Ad.*, Jan. 29, 1872. *Dis.*, May 30, „ *Result*, cured.	Compound depressed fracture of the right temporal bone. Patient quite sensible on admission.

Result of Accident (*continued*).

was successful in 22 instances, and none of these required further measures; all healed. But in 8, or 26·6 per cent., the attempt was unsuccessful; of these 1 died, in 2 secondary amputation was necessary, one of which patients also died of an independent disease, while in some of the remainder the further progress of the case was unsatisfactory. This is a striking fact, as showing the difference in results between cases treated aseptically and those treated with antiseptics. These figures do not, however, rightly represent the present probabilities of rendering compound fractures aseptic, for the majority of the failures occurred early in the treatment, while of late the greater number of attempts have been successful. Thus from the end of 1871 till the end of 1875, 18 cases were treated, and of these 6, or 33·3 per cent., failed. On the contrary, from the end of 1875 till 1880, 12 cases were treated, only 2 of which, or 16·6 per cent., failed. The chief improvement which has led to this greater success is no doubt the method of washing out the wound thoroughly by means of a catheter attached to a syringe—a method introduced since 1875. The chance of success depends, however, almost entirely on the nature of the wound and the amount of dirt in it. If it be complicated and dirty the result is doubtful; otherwise success is almost certain.

of the Skull (accidental).

Treatment	Remarks
Clots removed, and the wound syringed out with 1-20 carbolic lotion. Gauze dressing applied.	The wound remained aseptic, but in a few hours the patient became restless and complained of pain in his head. He soon became unconscious. Trephining was therefore performed on January 31, the depressed bone elevated and some fragments of the internal table removed. Patient recovered consciousness after the operation, and progressed rapidly towards recovery, the blood-clot lying exposed in the wound. At one part, at the time of the operation, the bone was felt bare for some distance, but none separated, and it was all completely covered on February 22. The wound had completely healed on May 2. When the patient was discharged, the pulsations of the brain could be seen to raise the cicatrix. (See T. Chart XXVI.)

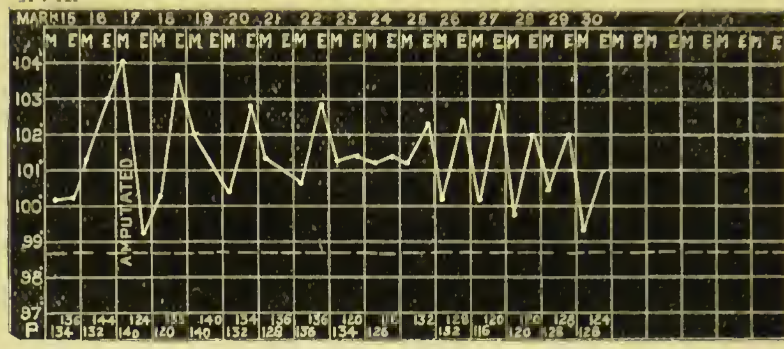
I.—COMPOUND FRACTURES, THE

No.	Name and Age	Date of Admission and Discharge; with Result	Injury
44	George S., 18 .	*Ad.*, Feb. 12, 1872. *Dis.*, Feb., 18, ,, *Result*, in process of cure.	Compound fracture of the os frontis. No depression.
45	J. McC. (adult)	*Ad.*, March 4, 1872. *Dis.*, May 2, ,, *Result*, cured.	The handle of a windlass struck patient on the forehead, causing a depressed compound comminuted fracture of the os frontis. Fracture of nasal bones. The patient was conscious when admitted.
46	J. McK., 7 . .	*Ad.*, June 14, 1872. *Dis.*, July 22, ,, *Result*, in process of cure.	Depressed compound fracture of temporal bone.
47	William L., 32	*Ad.*, Nov. 15, 1872. *Died*, Nov. 17, ,, The cause of death was injury to the brain substance.	Patient fell from a height of 20 feet, sustaining a compound fracture of the skull. Patient insensible, pupils dilated, surface cold, had several convulsive fits before admission.
48	Andrew P., 7 .	*Ad.*, Sept. 15, 1876. *Dis.*, Oct. 8, ,, *Result*, cured	Compound fracture of temporal and frontal bones. Bleeding from the ear and nose. Run over by the wheel of a cab, head markedly flattened.
49	G. W. E., 12 .	*Ad.*, April 2, 1878. *Dis.*, April 15, ,, *Result*, cured.	An iron gate fell on his head detaching nearly half the scalp, which was hanging down. Bone fissured.
50	George H., 36 .	*Ad.*, May 14, 1878. *Died*, May 15, ,, The cause of death was fracture of the skull, &c.	Patient fell 10 feet, striking his head and causing an extensive compound comminuted fracture of the skull. Patient insensible, but muttering and restless.
51	David McC., 30	*Ad.*, Oct. 3, 1880. *Dis.*, Oct. 18, ,, *Result*, cured.	Struck on the head by a bottle, causing two incised wounds and fissure of the skull. Patient drowsy, but sensible.
52	A. C., 15 . . .	*Ad.*, Jan. 22, 1880. *Dis.*, Feb. 9, ,, *Result*, in process of cure.	Fell on his head from a height of 35 feet. Large scalp wound, fracture of parietal bone, and other injuries, no depression. Patient quite insensible when admitted into hospital.
53	Thomas G., 32 .	*Ad.*, April 9, 1880. *Dis.*, April 20, ,, *Result*, cured.	Fell 68 feet, and struck his forehead, receiving a compound fissure of the frontal bone. Patient sensible; other bruises present.

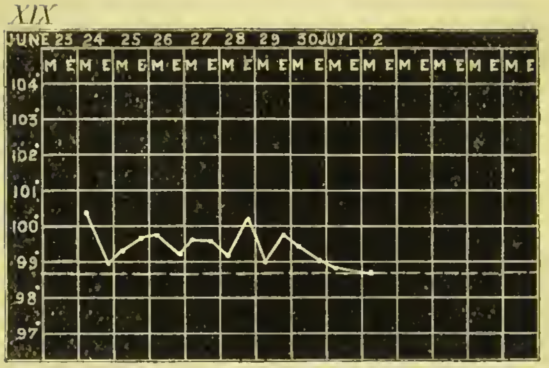
ESULT OF ACCIDENT (*continued*).

Treatment	Remarks
Vound injected with 1-20 carbolic lotion, and for the most part stitched.	Aseptic course. No suppuration, the wound healing by first intention and organisation of blood clot.
Vound injected with 1-20 carbolic lotion, depressed bone elevated, and portions of loose bone and of the internal table removed.	Aseptic course. The wound had quite healed on March 16. As there was slight redness and pain at one part of the scar, it was opened up on March 27, and one or two minims of pus escaped. This had soundly healed by April 19. There was an elevation of temperature on March 7, apparently due to derangement of the bowels, as it subsided at once on administering a laxative. (See T. Chart XXVII.)
Vound injected with 1-20 carbolic lotion, and left quite open.	This case followed an aseptic course till July 12, when, probably owing to the restlessness of the patient and consequent displacement of the dressings, the wound was found to have become putrid. By this time, however, the deeper parts had closed. When discharged there was still a small suppurating wound, and a portion of the os frontis could be felt to be bare.
omminuted portions removed, and depressed bone elevated. Wound washed out with 1-20 carbolic lotion.	Absolute insensibility, with occasional convulsive attacks, continued till his death, about 36 hours after the injury.
ounds injected with 1-20 carbolic lotion. Ears stuffed with gauze.	Aseptic course, except that an abscess formed on the side of the head behind the eye. Everything had healed, and was apparently sound when the patient was discharged.
ound thoroughly washed out with 1-20 carbolic lotion, catgut drain and numerous horsehair stitches.	Aseptic course. The wound healed entirely by first intention, except at one point where a little piece of horsehair drain was employed. On April 12, there were just three tiny points to heal. A collodion dressing was applied. (See T. Chart XXVIII.)
portion of loose bone was removed, and the wound was syringed out and left open.	The patient never recovered consciousness, and died about 27 hours after the accident. The skull was found to be very extensively fractured and hæmorrhage had occurred in various places but no lesion of the brain was found.
wo small pieces of the outer table were removed and the wound was washed out with 1-20 carbolic lotion.	Aseptic course. On October 13 two additional pieces of bone were removed. (See T. Chart XXIX.)
portion of bone was loose, and was removed, wound washed out with 1-20 carbolic lotion.	Aseptic course. The wound had almost healed when the patient was discharged, to be treated as an out-patient. (See T. Chart XXX.)
ound washed out with 1-20 carbolic lotion.	Aseptic course. The wound had quite healed when the patient was discharged.

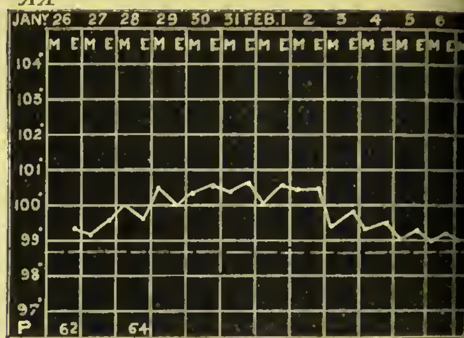
XVIII



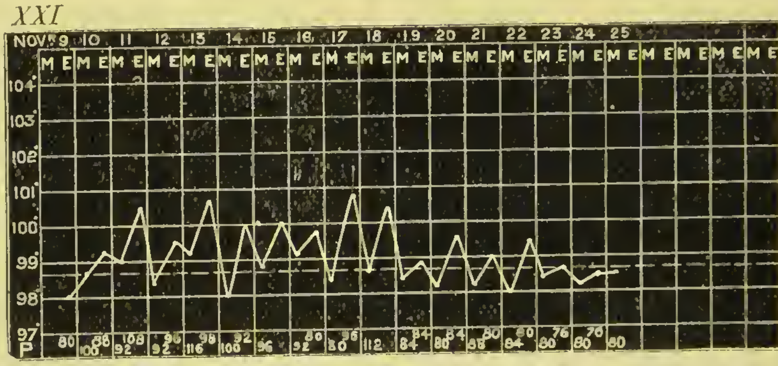
XIX



XX



XXI



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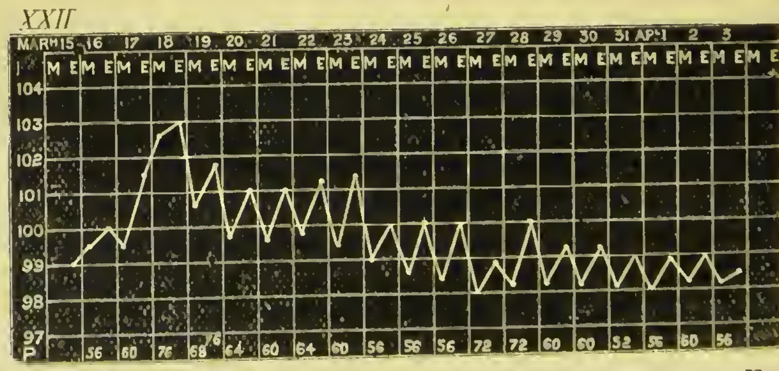
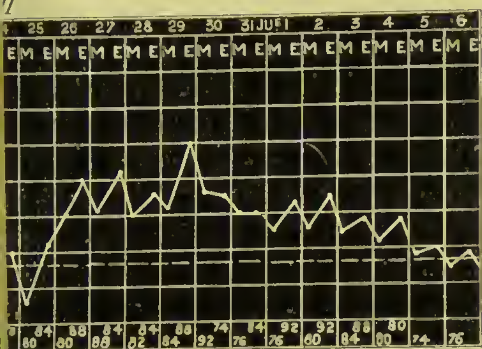


Temperature Charts of Mr. Lister's Cases of Compound Fracture.



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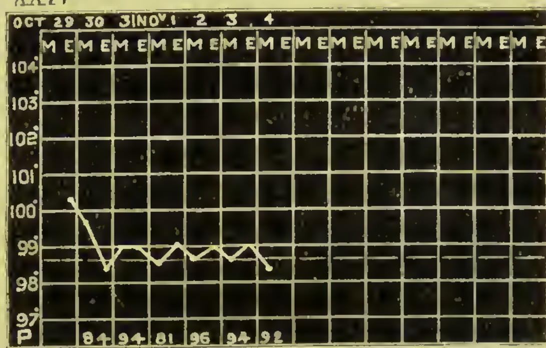
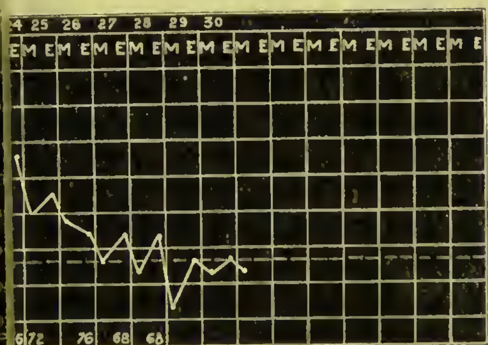
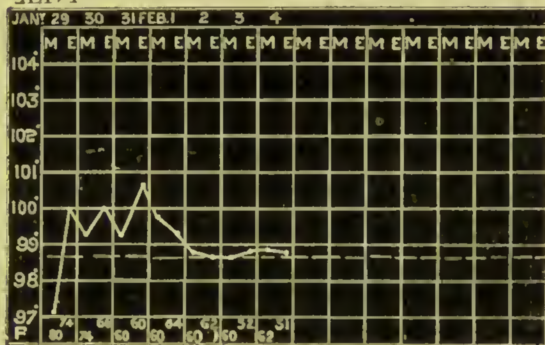
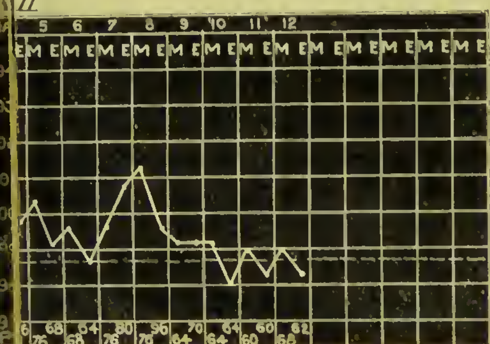


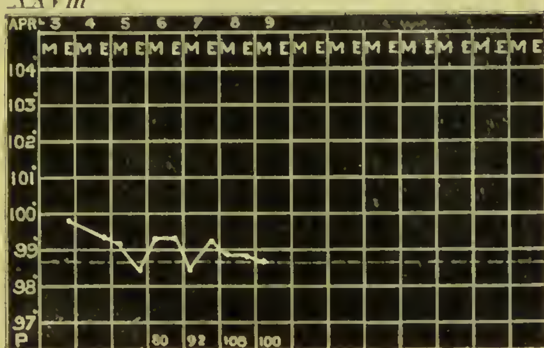


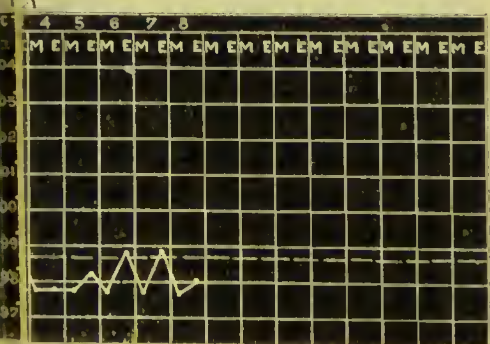
XXVI



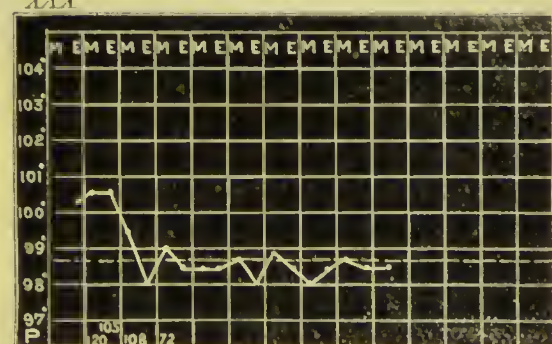


XXVIII





XXX



MPERATURE CHARTS OF MR. LISTER'S CASES OF COMPOUND FRACTURE (*cont.*)

There were thus 11 cases of compound fracture of the skull with two deaths, both due to the nature of the injury, and both occurring within forty-eight hours. In all the 9 cases which survived putrefaction was avoided, and the result was good.

The whole result of Mr. Lister's hospital practice in regard to compound fractures produced accidentally since 1871 is therefore as follows :—52 cases of compound fracture of the skull and long bones (53 fractures) were treated with 10 deaths.

Leaving out of consideration here all the cases which died within forty-eight hours, we have 44 cases (45 fractures) with 2 deaths, 1 from diphtheria and 1 from bronchitis and cardiac disease.

Or, taking the facts in another way, and, as is generally done, leaving out cases requiring primary amputation, we have 42 cases (43 fractures) treated conservatively with 6 deaths; and omitting also cases which died within 48 hours, and which have no bearing on the point at issue, we have 38 cases of compound fracture of the skull and long bones (39 fractures) with 2 deaths, 1 of these deaths being certainly independent of the wound.

In 31, or 81·5 per cent., of these cases the attempt to eradicate the causes of putrefaction was successful, and all of these cases recovered, *i.e.*, 31 compound fractures of the skull and of the long bones were treated aseptically without a death and, in the case of the limbs, without necessity for further operation.

I now propose to consider the compound fractures made by the

II. COMPOUND FRACTURES

Compound Fractures of

No.	Name and Age	Date of Admission, Operation and Discharge; with Result	Injury
54	James M., 13 .	*Ad.*, Jan. 22, 1873. Date of discharge not given. *Result*, cured.	Anchylosed knee-joint. Knee bent at right angles.
55	Alexander W., 37	*Op.*, June 18, 1873. *Result*, healed.	Ununited fracture of the femur-joint above the knee-joint.
56	Frances G., 54 .	See No. 6, p. 426. *Result*, healed.	Ununited fracture of the neck of the femur.

urgeon, and I would remark that Mr. Lister's osteotomies were in o sense of the word subcutaneous. They were real compound actures; the wound in the bone communicated freely with the xternal world and a drainage-tube in most cases kept up this ommunication after the operation. In some instances the bone was imply chiselled partly through and then broken; in others, portions f the bone were removed. I have not included in this list exci- ions of joints for disease, partly for the reasons stated on p. 380, and lso because in almost all the cases of diseased joints, sinuses are resent before the patients come into hospital, and therefore the cases annot as a rule be treated aseptically: 1 or 2 cases where a oint was anchylosed, and where a wedge-shaped piece of bone was emoved, in other words where a compound fracture was produced on *healthy* bone, are included.

From these cases the element of shock is excluded, and there- ore we ought to have results comparable to the 38 cases of compound acture with 2 deaths; and if these deaths were really independent f the wound, we ought here to have no deaths at all. Here also he certainty as to the ultimate result ought to be greater and the haracter of the results better; for here we have merely to *exclude* e causes of fermentation, whereas in the other cases we had to *estroy those which had entered.* In other respects they are omparable injuries, and, therefore, at the end I shall sum up all the ompound fractures together, whether they have been made by the irgeon or have occurred accidentally.

ADE BY THE SURGEON.

e Femur (*Surgeon*).

Treatment	Remarks
wedge-shaped piece of bone was removed, and the leg was brought straight. Incision made over the seat of fracture.	Typical aseptic course. On February 2 all the stitches were removed, and every thing was healed, except where the drainage-tube was. All soundly healed on February 11.
Ends of bones refreshed by gouge and hammer. Wound left open.	Aseptic course. Completely healed during August. The temperature was taken once daily, and was only once up to 100°. No union. See Nos. 57 and 58.
Operated on.	Aseptic course. (See T. Chart IV.)

II. COMPOUND FRACTURES

No.	Name and Age	Date of Operation and Discharge; with Result	Injury
57	Alexander W., 38	*Op.*, March 6, 1874. *Result*, healed.	See No. 55. Fracture still ununited.
58	Alexander W., 38	*Op.*, July 17, 1874. *Dis.*, Sept. 21, „ *Result*, cured.	See No. 57. Fragments still ununited.
59	Thomas D., 37 .	*Op.*, July 23, 1875. *Dis.*, Nov. 23, „ *Result*, healed.	Ununited fracture of femur about its middle, of eleven months' standing. Various methods of treatment tried.
60	Richard K., 12.	*Op.*, Oct. 4, 1875. *Dis.*, Dec. 14, „ *Result*, cured.	Aggravated knock-knee on one side.
61	Peter M., 30 .	*Op.*, Feb. 19, 1876. *Dis.*, July 27, „ *Result*, cured.	Ununited fracture of the femur about ten inches below the great trochanter. Of ten months standing.
62	C. W. Y., 9 . .	*Op.*, March 14, 1876. *Dis.*, Oct. 14, „ In process of cure.	Badly united fracture of the left femur just below the trochanters. Fracture occurred eighteen months previously. 1¾ inch shortening.
63	Thomas D., 38 .	*Op.*, July 19, 1876. *Dis.*, Sept. 20, 1877. In process of cure.	See No. 59. Fracture still ununited.
64	Mary McD., 16.	*Op.*, July 15, 1876. *Dis.*, Feb. 7, 1877. *Result*, cured.	Double knock-knee. Patient could hardly walk. The deformity commenced two years previously, after a fever.

[M]ADE BY THE SURGEON (*continued*).

Treatment	Remarks
Similar operation to the former. Fragments drilled in two places and iron pegs driven in. Two wounds made. Drainage-tube inserted.	The pegs became loose and were removed on March 16. The wound on the inner side healed April 29, and the other soon afterwards. Limb kept in plaster till June 8, and put up again till July 16, but still no union. The temperature was once up to 100° (100·1), on the morning after the operation.
A similar operation was again performed, and the leg was put up in plaster of Paris at once. (Intermediate amputation).	Putrefaction occurred here probably during the application of the plaster. As the temperature was going up, Mr. Lister thought it best to amputate. This was done on July 22, and the wound followed a typical aseptic course, healing entirely by first intention except where the drainage-tube was.
Ends of bone gouged. Drainage-tubes inserted.	Aseptic course. The blood-clot became organised as usual, and the gaping wound had completely cicatrised on September 12, without any pus-formation. Union had not occurred when the patient left hospital, wearing an immovable apparatus. (See T. Chart XXXI.) See No. 63.
An incision was made above the condyles of the femur down to the bone. Periosteum detached, and a wedge-shaped piece of the femur removed. Drainage-tubes inserted.	Typical aseptic course. No suppuration. Wound completely healed on November 14. Highest temperature was 99·6°. When seen some weeks after his discharge the femur was quite firm and the limb straight.
Incision on outer side of thigh. Ends of bone removed. Bones drilled and tied together with silver wire.	Aseptic course. Put up in plaster of Paris in April. Apparatus taken down on July 5. Bones united. Wire removed. (See T. Chart XXXII.)
Incision on the outer side of the thigh over the seat of fracture; bone divided; limb brought straight. Ends of bone refreshed and tied together with silver wire.	A good deal of constitutional disturbance followed the operation; the wound suppurated and abscesses formed. When the patient was discharged a small sinus remained unhealed. The femur was quite firm and the leg straight, and only ½ in. shorter than the other. One or two small pieces of bone came away. Ultimately healed.
Ends of bones refreshed and tied together with silver wire.	Aseptic course. Patient was kept in hospital having various immovable apparatuses applied at intervals, but when discharged there was still a little mobility. The silver wire was left in, and the fragments ultimately united without further operation. (See T. Chart XXXIII.)
A wedge-shaped piece of bone was taken out of each femur on the same day. (See No. 60.) Drainage-tubes inserted.	Aseptic course. The right leg had completely healed at the end of August, and the left almost. Both limbs were put up in plaster of Paris on August 30. This was taken down on November 30, when both bones were found to be united (See T. Chart XXXIV.)

II. COMPOUND FRACTURES

No.	Name and Age	Date of Operation and Discharge; with Result	Injury
65	Adam S., 6 . .	*Op.*, Aug. 1, 1876. *Dis.*, Dec. 4, „ *Result*, cured.	Aggravated knock-knee on one side.
66	John T., 11 . .	*Op.*, June 29, 1877. *Dis.* during autumn. *Result*, cured.	Both knee-joints anchylosed at an acute angle—the result of previous disease.
67	Emma P., 9 . .	*Op.*, Nov. 20, 1877. *Dis.*, July 27, 1878. *Result*, cured.	Left knee-joint anchylosed nearly at a right angle.
68	George E., 12 .	*Op.*, Jan. 29, 1879. *Dis.*, Dec. 14, „ *Result*, healed.	Ununited fracture of the neck of the femur of six weeks' standing (extracapsular).
69	Thomas E., 35 .	*Op.*, March 19, 1879. *Dis.* May 15, „ *Result*, cured.	Double genu valgum.
70	Jane D., 50 . .	*Op.*, April 9, 1879. *Dis.*, June 30, „ *Result*, healed.	Ununited fracture of the femur.
71	Beatrice J., 14 .	*Op.*, May 16, 1879. *Dis.*, July 8, „ *Result*, cured.	Genu valgum on the right side.
72	Frank J., 7 . .	*Op.* May 21, 1879. *Dis.*, July 8, „ *Result*, cured.	Genu valgum on both sides.

IADE BY THE SURGEON (*continued*).

Treatment	Remarks
Similar operation to No. 64. Drainage-tube inserted.	Aseptic course. Wounds quite superficial and almost healed on September 1. The femur was quite firm on September 4, but there was still slight divergence outwards. This was due to want of care in adjusting the splints during the holidays.
Left leg operated on. Only so much bone removed as was necessary to obtain a straight position of the limb.	Aseptic course. The greater part of the wound healed by first intention, but two sinuses were still unhealed in the beginning of September when Mr. Lister left Edinburgh. I hear that the patient left the hospital soon afterwards, with the wounds quite healed and able to move his knee very slightly (a movable knee-joint was aimed at in the first instance).
Portions of the end of the femur were removed, and the leg was got straight after division of the hamstring tendons. Horsehair drain.	Aseptic course. Wound healing by first intention except where the drain was. The drain was removed on January 12. A small abscess formed on the outer side of the knee, and was opened on February 5. A minute sinus remained here for a long time, the wound being soundly healed and the bones firm. It healed a few days after the patient was sent to a convalescent home.
A long incision was made on the outer side of the thigh over the great trochanter. The fragments refreshed and two drainage-tubes inserted. Long splint.	There was considerable constitutional disturbance after the operation, and the temperature at times was as high as 101·6°. A little suppuration occurred from the wound, but the greater part healed by first intention. A little bit of loose dead bone (apparently a chip from the operation) was removed on November 11, and then the sinus at once closed. Union did not occur. From the high temperature and the suppuration it is probable that some ferment had got in, but the discharge was not examined, and it never had any smell. (See T. Chart XXXV.)
MacEwen's operation on both thighs at the same time. Drainage tubes inserted.	Aseptic course. Both wounds had healed at the end of April. Union was then complete. Patient began to walk on May 5. (See T. Chart XXXVI.)
Ends of fragments refreshed and tied together with silver wire.	Aseptic course. No suppuration. Wound healed about May 15. Patient was discharged wearing an apparatus, but union did not occur.
MacEwen's operation on the right thigh. Drainage-tubes inserted.	Aseptic course. Healed about June 5. Splints removed and bones united on June 28. Began to walk on June 29. (See T. Chart XXXVII.)
MacEwen's operation on both limbs on the same day. Drainage-tubes inserted.	Aseptic course. Both wounds had healed by June 10. Union perfect on June 30. Joints freely movable. (See T. Chart XXXVIII.)

II. Compound Fractures

No.	Name and Age	Date of Operation and Discharge; with Result	Injury
73	Lydia W., 9 .	*Op.*, July 9, 1879. *Dis.*, Aug. 19, „ *Result*, cured.	Genu valgum on right side. See also No. 87.
74	Ethel S., 5 . .	*Op.*, Oct. 15, 1879. *Dis.*, Dec. 14, „ *Result*, cured.	Double genu valgum.
75	John M., 15 .	*Op.*, Oct. 22, 1880. *Dis.*, Jan. 3, 1881. *Result*, cured.	Genu valgum on the right side.
76	Henry D., 17 .	*Op.*, Nov. 5, 1880. *Dis.*, Jan. 7, 1881. *Result*, cured.	Bony anchylosis of knee-joint of eight years' standing: leg at right angles to the thigh.

We have thus 23 cases in which compound fractures of the femur have been made by the surgeon (27 compound fractures). In all but 3 cases there was a typical aseptic course, *i.e.*, the patient ate as well and was as well in every respect as if he had not been operated on, while there was no suppuration from the deeper parts of the wound,

Compound Fractures

77	John C., 44 . .	*Op.*, Aug. 28, 1872. *Result*, healed.	Ununited fracture of both bones of the leg.
78	John C., 45 . .	*Op.*, July 30, 1873. *Result*, healed.	Former case (No. 77). Still ununited.
79	John C., 45 . .	*Op.*, Jan. 15, 1874. *Dis.*, Sept. 3, „ *Result*, cured.	Former case (Nos. 77 and 78). Tibia still ununited.
80	Eliza L., 23 . .	*Op.*, June 6, 1874. *Dis.*, Aug. 13, „ *Result*, cured.	Badly united Pott's fracture. Leg much everted.
81	Eliza L., 24 . .	*Op.*, March 17, 1875. Exact date of discharge cannot be ascertained. *Result*, cured.	Patient (No. 80) had used her foot too freely, and there was some return of the deformity.

IADE BY THE SURGEON (*continued*).

Treatment	Remarks
MacEwen's operation. Drainage-tube inserted.	Aseptic course. No date of healing. Allowed to get up for the first time on August 12. Union then perfect and wound healed.
MacEwen's operation on both sides. Drainage-tubes inserted.	No local disturbance, but the temperature rose a little for a day or two, being once as high as 101°. The child, however, was quite well. On November 6 one wound was healed and the other nearly so. Both legs seemed to be firm at that time. (See T. Chart XXXIX.)
MacEwen's operation on the right side.	Aseptic course. Wound superficial on November 10, and boracic dressing applied. Patient got up on December 4. (See T. Chart XL.)
Wedge-shaped piece of bone removed. Horse-hair drain.	Aseptic course. On December 9 all healed except two spots where the drain was. Patient got up on January 2. Seen last on March 20, when he could walk well without any support. Bones quite firm. (See T. Chart XLI.)

nd as a rule none at all from the superficial part. In one case where putrefaction occurred, secondary amputation was performed as eing the safest treatment and also the best when the difficulty in etting union and the shortening of the limb were taken into ccount.

f the Leg (*Surgeon*).

Ends of the bones refreshed and brought into good position. Wound left open.	Aseptic course. On November 5 the limb was put up in plaster of Paris. This was reapplied several times till July, 1873, but union did not take place. See No. 78.
Similar operation.	Aseptic course On September 5 the wound was small and quite superficial. The fibula united, but the tibia did not. See No. 79.
Similar operation. Iron pegs were driven into the tibia.	Aseptic course. On March 12 union was found to have occurred, and the pegs were removed. The rest of the wound had healed. The patient was, however, kept in hospital for some time.
Fibula divided obliquely. Foot brought straight. Wound left open. Dupuytren's splint.	Aseptic course. Wound had healed and the bones were apparently firm when the patient was discharged.
Similar treatment.	Aseptic course. Kept longer in an apparatus. On this occasion the cure was permanent. (See T. Chart XLII.)

II. Compound Fractures

No.	Name and Age	Date of Operation and Discharge; with Result	Injury
82	Martha C., 28 .	*Op.*, Aug. 18, 1875. *Dis.*, Oct. 2, 1875. *Result*, healed.	Congenital deformity of right foot. Inversion of foot, the inner surface of which formed an angle of 140° with the axis of the leg.
83	William M., 36	*Op.*, Dec. 21, 1875. *Dis.*, March 2, 1876. *Result*, cured.	Badly united fracture of the leg with displacement of the foot backwards. Of fourteen months' standing.
84	Martha C., 28 .	*Op.*, Feb. 27, 1876. *Result*, cured.	See case No. 82.
85	Finlay McD., 29.	*Op.*, Jan. 16, 1877. *Dis.*, June 9, „ *Result*, cured.	Badly united fracture of both bones of the leg, just below the tuberosity of the tibia. Leg bent inwards.
86	Alexander A., 23	*Op.*, July 19, 1877. *Dis.*, Oct. 13, „ *Result*, cured.	Ununited fracture of both bones of the leg at the junction of the middle and lower thirds. Of fourteen weeks' standing.
87	Lydia W., 9 .	*Op.*, July 9, 1879. *Dis.*, Aug. 19, „ *Result*, cured.	Rickety deformity of one leg. See also No. 73.
88	Henry B., 32 .	*Op.*, Dec. 12, 1879. *Dis.*, March 31, 1880. *Result*, cured.	Badly united Pott's fracture. Foot much everted.
89	Frederick A., 26	*Op.*, Nov. 6, 1879. *Dis.*, Dec. 16, 1879. *Result*, cured.	Patient was admitted a fortnight previously with simple fracture of both bones of the leg. A fragment of the tibia projected under the skin, causing great pain and threatening to protrude.
90	Joseph B., 2. .	*Op.*, June 18, 1880. *Dis.*, July 15, 1880. *Result*, cured.	Rickety deformity of both legs.

ADE BY THE SURGEON (*continued*).

Treatment	Remarks
Fibula cut through and a wedge-shaped portion of bone removed from the tibia. Extensor tendons divided. Bones tied together by silver wire. Foot brought into straight position.	Aseptic course. Wounds superficial on September 18. When seen in February, 1876, the bone was still ununited. See No. 84. (See T. Chart XLIII.)
Tibia and fibula divided by lateral incisions. Foot brought straight. Dupuytren and horseshoe splints.	Aseptic course. Bones firmly united and only a little speck to heal on February 1.
Ends of fragments refreshed. Similar after-treatment to that formerly adopted.	Aseptic course. On this occasion union took place, but the dates of union and discharge are not given in the note books.
Fibula cut across. Wedge-shaped piece of bone removed from the tibia. A portion of the fibula cut away. Leg straightened, and wounds left open.	Aseptic course. Tibial wound found healed on March 1, and the fibular on March 3. Plaster of Paris removed on March 10, and the bones found to be firm. It was reapplied till May 1, when it was finally left off, the bones being quite firm and strong. (See T. Chart XLIV.)
Incisions over each bone, and the fragments refreshed. Iron pegs driven into the fragments of the tibia.	Aseptic course. The temperature rose on one occasion as high as 99·8°. Iron pegs removed on September 1, and wounds healed a few days later. On October 4 the bones were found to be quite united.
Incisions over the bones which were then cut across. Drainage-tubes inserted. No stitches.	Aseptic course. Date of healing not given. Patient allowed to get up for the first time on August 12. Union then perfect and the wounds quite healed.
An incision was made over the site of the former fracture, and the fibula divided obliquely. Foot inverted by means of pulleys. Drainage-tubes inserted. Dupuytren's splint.	Aseptic course. Wound quite superficial and boracic dressing applied on January 16. Splint left off and patient allowed to get up on February 24. There was a little pointing of the toes, which was overcome by elastic force. (See T. Chart XLV.)
Mr. Lister cut down on and removed the projecting portion, thus causing a compound fracture.	Aseptic course. On November 17 the wound was quite superficial, and boracic dressing was applied. The wound was healed and the bones firm when the patient left the hospital.
Incisions over the tibia and fibula. Bones divided. Wounds left open. Both legs operated on.	Aseptic course. No date of healing. Wounds quite healed and the bones firm when the patient was discharged. (See T. Chart XLVI.)

II. Compound Fractures

No.	Name and Age	Date of Operation and Discharge; with Result	Injury
91	Jessie C., $2\frac{1}{2}$.	*Op.*, Oct. 29, 1880. *Dis.*, Dec. 20, ,, *Result*, cured.	Bad rickety deformity of both legs.

Fifteen patients with compound fracture of the leg were treated without a death; and in these cases there were 31 separate compound

Compound Fractures of

92	Henry F., 16 .	*Op.*, Feb. 4, 1875. *Dis.*, March 2, ,, *Result*, cured.	Simple fracture of the clavicle; a fragment projecting under the skin, causing great pain and interfering with the adjustment of the fracture.
93	William A., 71	*Op.*, June 1, 1877. *Dis.*, Aug. 6, ,, *Result*, cured.	Unreduced dislocation of the acromial end of the clavicle. Of ten weeks' standing. Patient cannot use the arm well.
94	Elizabeth H., 22	*Op.*, Feb. 12, 1879. *Dis.*, April 12, ,, In process of cure	Cervical rib pressing forward the brachial plexus and causing great pain. Patient very hysterical.

There were 3 compound fractures of the clavicle without any bad

Compound Fractures of

95	Alexander S., 50	*Op.*, Feb. 9, 1872. *Dis.*, Sept. 5, ,, In process of cure.	Ununited fracture of the humerus of two years' standing. Various methods of treatment had been previously adopted.
96	James McB., 48	*Op.*, Feb. 12, 1872. *Dis.*, Oct. 10, ,, *Result*, cured.	Ununited fracture of the humerus at the junction of the upper and middle thirds. Of six months' standing.
97	John B., 14 . .	*Op.*, Dec. 6, 1872. *Dis.*, Feb. 11, 1873. *Result*, cured.	Fracture of lower end of humerus with dislocation. Of five months' standing.

IADE BY THE SURGEON (*continued*).

Treatment	Remarks
Both bones in both legs divided, and wedge-shaped portions removed from the tibia. On the right side the bones were divided in two places.	Aseptic course. Wounds superficial and spray stopped on November 6. Exact date of healing not given; it was apparently about the end of November. Splints removed and the patient allowed to walk on December 14. (See T. Chart XLVII.)

ractures made. In no instance did any local or constitutional isturbance follow the operation.

e Clavicle (Surgeon).

Mr. Lister cut down and removed this loose portion, thus converting the case into one of compound fracture. Drainage-tube inserted.	Aseptic course. The wound had healed and the bone firmly united on March 6.
Articular surfaces removed and the ends of the bone tied together with silver wire. Wound left open.	Aseptic course. Wire removed on July 15. Healed on July 24. Union was perfect and the movements were greatly improved when the patient left hospital. (See T. Chart XLVIII.)
Mr. Lister cut down and divided the clavicle. Drawing aside vessels, &c., he removed the rib, which was attached to the sixth cervical vertebra, and articulated with the first and second ribs in front. Clavicle tied with silver wire. Drainage-tubes inserted.	Aseptic course. Wound healed by first intention, except where the drainage-tube was. Wound quite healed on March 12. The clavicle did not unite firmly till September. Wire removed during November. (See T. Chart XLIX.)

ymptom.

e Humerus (Surgeon).

Ends of fragments sawn off, bones tied together by silver wire. Wound left open.	Aseptic course. Wire removed during March, and the wound quite healed on April 4. The bone was almost, but not absolutely, firm, and the patient was discharged wearing a silicate apparatus.
Ends of fragments refreshed and tied together with silver wire. Drainage-tube inserted.	Aseptic course. The greater part of the wound healed typically, but a sinus remained around the wire leading down to bare bone. On June 3, osseous union was complete. Wire removed on August 23. When discharged there was still a sinus leading down to bare bone.
Excision of elbow-joint by a longitudinal incision.	No constitutional disturbance. A small abscess formed on the outer part of the limb, and was opened on December 20. It had healed on January 20. The operation wound had entirely healed on January 6, except a minute superficial crack with the healing of which the movement of the arm seemed for some time to interfere. The movement was good when patient was discharged.

II. Compound Fractures

No.	Name and Age	Date of Operation and Discharge; with Result	Injury
98	Peter B., 11.	*Op.*, Dec. 2, 1872. *Dis.*, Feb. 11, 1873. *Result*, cured.	Similar injury to that in No. 97.
99	Donald McL. 45.	*Op.*, Nov. 14, 1874. *Dis.*, Feb. 28, 1875. *Result*, cured.	Ununited fracture of humerus a little above its middle. Of fifteen months' standing.
100	James J., 24	*Op.*, Dec. 15, 1875. *Dis.*, June 8, 1876. In process of cure.	Fracture of lower end of humerus, with inability to use the arm. Done six weeks previously.
101	John N., 15 . .	*Op.*, Nov. 27, 1875. *Dis.*, Jan. 31, 1876. *Result*, cured.	Badly united fracture of the humerus, about its middle, the arm being bent inwards.
102	Jessie S., 14 .	*Op.*, Aug. 21, 1875. Date of discharge is not given. *Result*, cured.	Osseous anchylosis of the elbow-joint in the straight position, the result of old fracture.
103	Edward W., 12	*Op.*, Jan. 15, 1879. In process of cure.	Badly united fracture of humerus. See Operations on Joints, No. 18, p. 432.

Thus we have 9 compound fractures of the humerus without

Compound Fractures of

No.	Name and Age	Date of Operation and Discharge; with Result	Injury
104	John McI. 34 .	*Result*, cured.	Ununited fracture of olecranon. See Operations on Joints, No. 3, p. 426.
105	Thomas W., 14.	*Op.*, May 3, 1873. *Dis.*, Nov. 20, ,, *Result*, cured.	Ununited fracture of radius. See No. 38, p. 476.
106	Alexander —, 26	*Op.*, Nov. 10, 1875. *Result*, cured.	Dislocation of the head of the radius backwards. See Operations on Joints, No. 10. p. 428.
107	J. McJ., 30 . .	*Op.*, Jan. 15, 1876. *Dis.*, April 13, ,, *Result*, cured.	Ununited fracture of the radius: of sixteen months' standing. Had been previously operated on by another surgeon. The ends of the bones were not in contact.
108	James S., 53 .	*Op.*, Jan. 26, 1877. *Dis.*, June 19, ,, *Result*, cured.	Ununited fracture of the radius: of twenty-two weeks' standing.

ADE BY THE SURGEON (*continued*).

Treatment	Remarks
Excision of elbow-joint by a longitudinal incision.	Aseptic course, except that the drainage was not good for the first day or two. The wound had completely healed on January 27. Movements good when discharged.
Ends of fragments refreshed and tied together with silver wire. Wound left open.	Aseptic course. Wound quite healed on December 27. Union was complete and wire removed on February 27.
xcision of elbow-joint by longitudinal incision.	The wound went on well till the Christmas holidays, when it putrefied. After that an abscess formed in the upper arm. In March only two sinuses remained to heal. It is not stated whether these had completely healed when patient was discharged. Movements fair.
one divided by hammer and chisel and brought into proper position. Drainage-tube inserted and plaster of Paris applied at once.	Aseptic course. The wound had completely healed on January 16. The bones were firm when patient was discharged.
xcision of elbow by H-incision.	Aseptic course. The incisions were quite healed on September 25. Movements good. (See T. Chart L.)
	Aseptic course.

ly bad results.

e Forearm (Surgeon).

Treatment	Remarks
	Aseptic course.
piece of bone was cut out of the ulna to allow the ends of the radius to come into contact. The ends of the radius were refreshed Fragments tied together by silver wire. Left open.	Aseptic course. Plaster of Paris applied on July 28, when both wounds were healed. Removed on November 17, when union was complete. Wires also removed on that day. Seen again in March, 1874, when the arm was strong, and in every way perfectly useful.
	Aseptic course.
lna divided and a portion removed. The ends of the radius refreshed. Fragments tied together with silver wire. Wounds left open.	Aseptic course. The wounds had healed and union was complete when the patient was discharged.
nds of radius refreshed. A piece cut out of the ulna. Fragments tied together with silver wire. Wounds left open.	Aseptic course. Both wounds had healed in six weeks without any pus formation whatever. Plaster of Paris was applied on March 3. Apparatus removed on June 2, when union was complete. Wires removed on that day. (See T. Chart LI.)

II. COMPOUND FRACTURE

No.	Name and Age	Date of Operation and Discharge; with Result	Injury
109	William B., 45	*Op.*, March 20, 1879. *Result*, cured.	Ununited fracture of the olecranon. See Operations on Joints, No. 17, p. 432.
110	John H. 28 . .	*Op.*, Jan. 14, 1881. *Dis.*, March 5, ,, *Result*, cured.	Ununited fracture of both bones of the fore-arm, the result of a machine accident eleven months' previously.

In all there were 7 cases of compound fracture of the fore-arm with

Compound Fracture o

111	Maggie C., 17 .	*Op.*, April 1, 1875. *Dis.*, May 12, ,, *Result*, improved. One case of compound fracture of the lower jaw, which did well.	Anchylosis of one side of the jaw. Jaws firmly closed. The result of old necrosis.

Following the example of most writers who discuss the results of compound fracture, I shall now leave out of consideration the cases in which primary amputation was performed, and also those which died within forty-eight hours. The following result will then be obtained. Since the end of 1871 up to the present time Mr. Lister has had under his care 95 [1] patients affected with compound fractures; of these 95 patients only 2 died. (The number is apparently 97, but 2 patients are each counted twice.) I may tabulate the result as follows:

Part injured	No. of cases	No. of deaths	Total No. of fractures	Secondary amputations
Femur . . .	26	1	30	1
Bones of leg . .	34	1	50	2
Clavicle . .	3	—	3	—
Humerus .	12	—	12	—
Fore-arm . .	12	—	16	—
Skull . . .	9	—	9	—
Lower jaw . .	1	—	1	—
Total .	97	2	121	3

[1] A patient on whom an operation has been performed, whose wound has healed, and on whom a second operation has then been done, is, of course, reckoned as two separate cases

MADE BY THE SURGEON (*continued*).

Treatment	Remarks
	Aseptic course. (See T. Chart XI., p. 439.)
Incisions over each bone. Fragments refreshed and tied together with silver wire. Wounds left open.	Wounds superficial and spray discontinued on January 31. Silicate apparatus applied on February 7. Wires removed March 16. Union satisfactory. (See T. Chart LII.)

out any bad result. (There were 11 distinct compound fractures.)

the Lower Jaw (*Surgeon*).

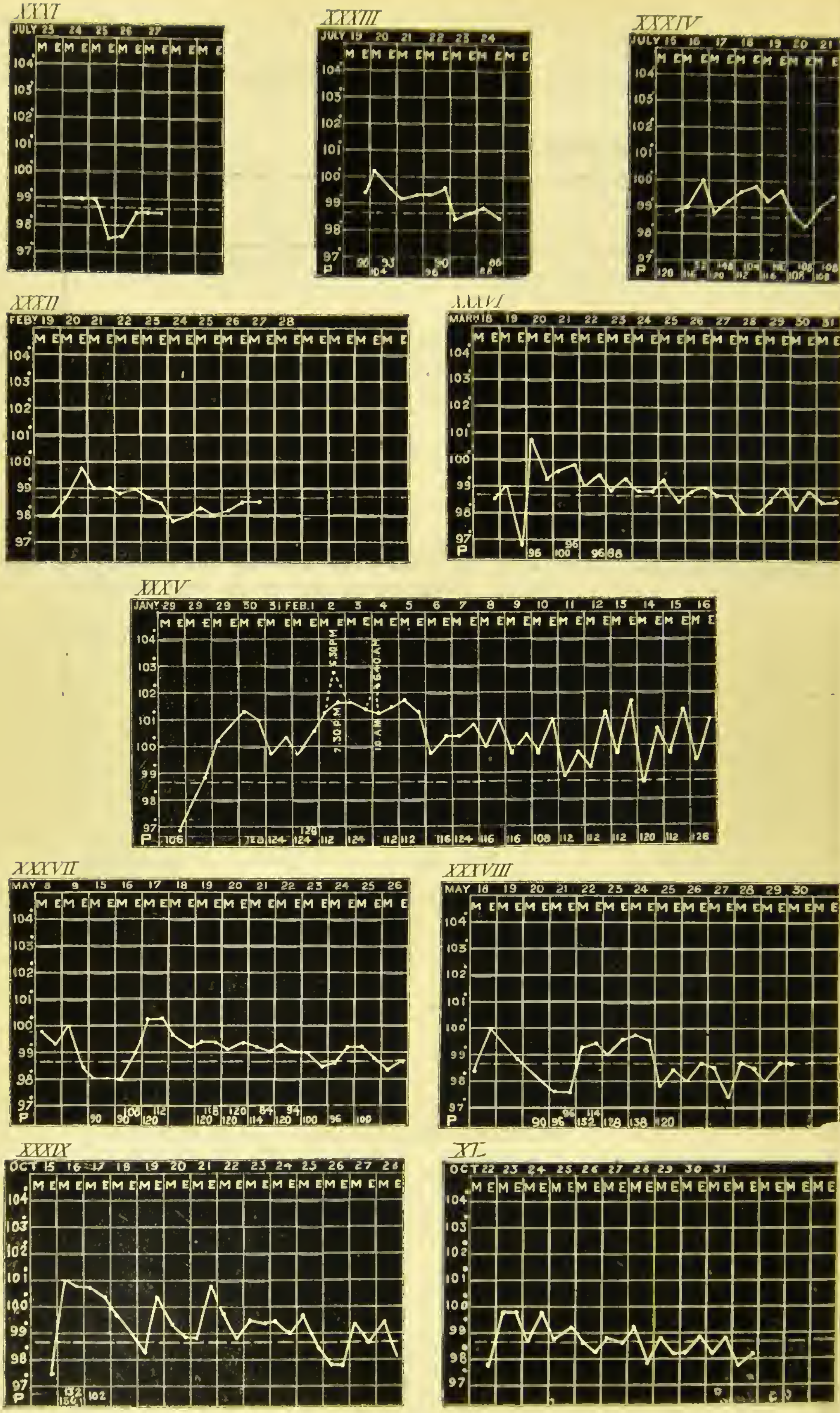
An incision was made behind the ramus of the jaw on the anchylosed side, and the condyle of the jaw was cut through.	The wound was almost absolutely healed on April 21, when boracic dressings were applied. A wedge was kept between the teeth, and when the patient was discharged she could open her mouth without aid for half an inch. When seen in July her condition had much improved.

It may, perhaps, be more convenient if I separate the cases of accidental compound fracture from those made by the surgeon. The following is the list of the accidental ones :—

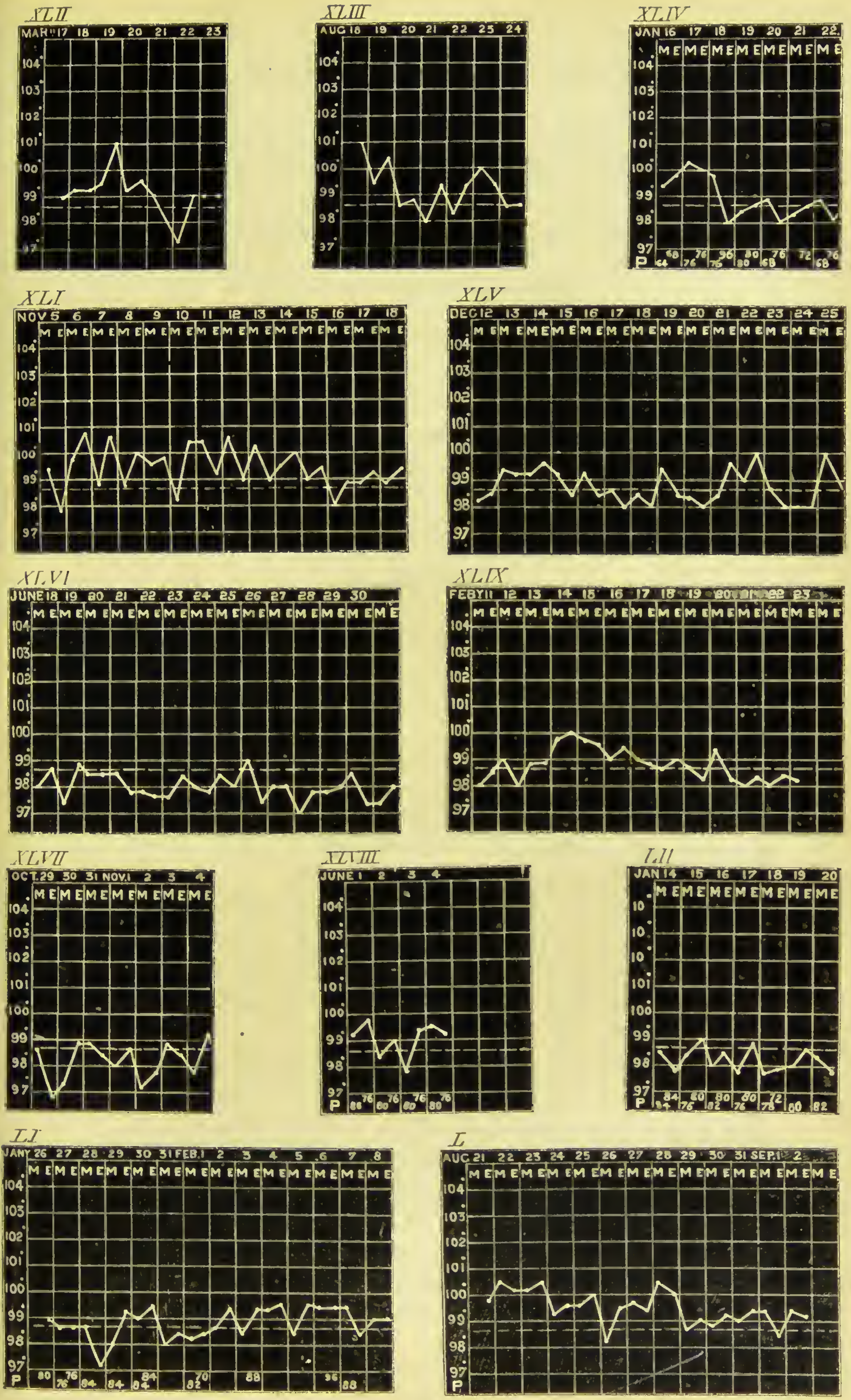
Bone	No. of cases	No. of deaths	Secondary amputations
Femur . . .	3	1	—
Leg	19	1	2
Humerus . . .	3	—	—
Fore-arm . . .	5	—	—
Skull	9	—	—
Total .	39	2	2

The following is the list of those made by the surgeon :—

Bone	No. of cases	No. of fractures	No. of deaths	Secondary amputations
Femur . .	23	27	—	1
Leg . . .	15	31	—	—
Clavicle . .	3	3	—	—
Humerus . .	9	9	—	—
Fore-arm . .	7	11	—	—
Lower jaw .	1	1	—	—
Total .	58	82	0	1



Temperature Charts of Mr. Lister's Cases of Compound Fracture.



TEMPERATURE CHARTS OF MR. LISTER'S CASES OF COMPOUND FRACTURE (*continued*).

Some writers have a fancy now-a-days to group the cases according to age. The following are the facts in these 95 cases :—

0–9 years	10–19	20–29	30–39	40–49	50–59	60–69	70–79
12 cases	25	19	20	8	7	2	1

(In one case the age is not known.)

If we again subdivide these into accidental and intentional, we find the following :—

Accidental.

0–9 years	10–19	20–29	30–39	40–49	50–59	60–69	70–79
4	10 (1 death)	7 (1 death)	9	2	3	2	0

(In one case the age is not known.)

Intentional.

0–9 years	10–19	20–29	30–39	40–49	50–59	60–69	70–79
8	15	12	11	6	4	0	1

It is thus evident that the results are not due to absence of old patients.

The result exactly confirms the anticipations expressed on p. 461, that where the element of shock is excluded, and where the surgeon has merely to *keep out* the causes of fermentation, not to *eradicate* them, the mortality would fall and an element of certainty be introduced into the results.

In order to get comparative statistics similar to those given in Chapter XVII., I have looked through Mr. Spence's account of his work from 1872 to 1878, given in the papers to which reference has already been made (p. 378), and the following seem to be the facts :—

I find mention of 16 primary amputations during this period, and of these 7 died. In only 1 of these cases is it stated that death occurred within forty-eight hours, and thus we have to consider 15 primary amputations with 6 deaths. Then I find 5 secondary amputations with two deaths, neither within forty-eight hours. Also 1 secondary excision which recovered. And then only 2 cases of compound fracture treated conservatively, both of which recovered. Whether these are all the cases of compound fracture treated conservatively, I

cannot say, but as these 2 are mentioned, I presume that had there been any more, they would also have been stated. Hence, in Mr. Spence's list of cases treated during the greater part of the period referred to, and in the same hospital as Mr. Lister's cases, we find traces (for they are not grouped) of 23 compound fractures which lived for more than forty-eight hours, and of these 8 died, some of the deaths being due to infective disease. These facts are also interesting as showing what a large proportion of compound fractures are amputated primarily in the practice of a surgeon not treating his cases aseptically, and also the large proportion of secondary operations. For of 23 compound fractures 15 were amputated primarily (6 deaths), 5 were amputated and 1 excised secondarily, and only 2 were treated conservatively. And even if Mr. Spence has not published all his cases of compound fractures the facts remain equally striking, for, with a much smaller number of patients than Mr. Lister and with less hospital accommodation, Mr. Spence has performed a larger number of primary and secondary operations.

But from these papers I glean the following facts which are free from any objection. During this time (1872–78), 6 compound fractures seem to have been made on healthy bones by Mr. Spence, and of these 2 died. One case, however, died in 36 hours, and therefore, following our rule, we have 5 intentional compound fractures of healthy bones with 1 death. These were, 1 excision of the head of the humerus in a case of unreduced dislocation—death in thirty hours; 1 excision of the knee for bad anchylosis; 1 operation for badly united fracture of the femur; and 3 excisions of the elbow for anchylosis—1 of these cases subsequently underwent amputation and died, apparently of septicæmia.

Before leaving Mr. Lister's results, I may refer to some operations on bones which have the same dangers as compound fractures, though the bone is not fractured across.

Thus the removal of exostoses is a serious matter. Mr. Lister has operated for exostoses thirteen times (from the end of 1871 till October 1880), four times on the femur, and twice on the tibia, without any bad result.

In chronic osteitis not yielding to treatment, Mr. Lister

cuts down, exposes the bone for a considerable extent, and then digs a long deep trough in it with the gouge and hammer. This treatment at once relieves the pain, and generally cures the disease. Mr. Lister has performed 10 such operations (from the end of 1871 to October 1880), without the slightest bad result in any instance.

And then there are a number of operations in which, as in the removal of tumours, portions of bone have been cut away without any bad result.

By including all these operations together the number of cases could be very much increased without the addition of any bad result. I have preferred, however, to adhere rigidly to the cases of compound fracture, meaning by that not subcutaneous division of the bone, but a large wound open and communicating freely with the bone, generally by means of a drainage-tube.

I shall now consider the results obtained by other surgeons, and I shall take first, as being the most remarkable results as yet published, the statements made by Dr. MacEwen in the 'Lancet' for September 18, 1880. He there gives the result of all the cases in which he has made compound fractures aseptically. He points out that these cases were really compound fractures, and not in any sense could they be called subcutaneous operations. The wounds varied in length from $\frac{3}{4}$ inch to $1\frac{1}{2}$ inch, and the edges of the wound were held aside so that the bone was freely exposed, and purified air had free access to the divided fragments. In this way he has operated on 330 patients affected with various deformities. Of these 220 had knock knee (367 limbs), 64 had bow legs (104 legs), 40 had tibial curves &c. (80 legs), and 6 had osseous anchylosis of the hip or knee The bones were either simply divided by the chisel and hammer, or wedge-shaped portions were cut out. Although he only operated on 330 patients, he produced compound fractures on 557 limbs. And as he often produced 2 or more separate compound fractures on the same limb, it came about that he had in all made and treated aseptically no less than 835 compound fractures.

What were the local results? Here we have to consider 835 compound fractures, and here Dr. MacEwen tells us that only in 8 out of these 835 wounds did suppuration occur, even

though union by first intention was impossible. In one case there was no apparent cause for the suppuration ; in the other 7 cases some cause was present. In 3 instances irritation and inflammation were set up by the pressure of the splint, &c. ; in 3 cases there was bruising and laceration of the soft parts ; and in 1 a piece of muscle was severely injured in adjusting the bones. In each of these 8 cases the occurrence of suppuration was preceded by elevation of temperature.

What was the result as regards the necessity for further operation? Here we have to speak of 557 limbs ; 557 cases in which secondary amputation might have been necessary. Secondary amputation was, however, only necessary in one instance, although some of the limbs had as many as four or five compound fractures. The operation in this case was necessary because, owing to an accident, the bandages had become displaced, and gangrene of the foot occurred. This patient recovered perfectly.

What were the results as regards life? Here we have to consider 330 lives which might have been lost, and in connection with this it is to be remembered that many of these patients had multiple compound fractures, in some instances as many as 10. And yet of these 330 cases only 3 died, and not one of these deaths was in any way due to the operation. MacEwen describes these 3 cases in detail, and I may state that the causes of death were diphtheria, tubercular meningitis, and pneumonia contracted before the operation.

To sum up Dr. MacEwen's results, we find that of 330 patients only 3 died, in each case the fatal result being independent of the disease : of 557 limbs affected with compound fracture only 1 required secondary amputation or other operation, and this from a cause independent of the wound ; and lastly, of 835 compound fractures, suppuration only occurred in 8 wounds, though primary union was impossible in all.

These results are the more striking when it is remembered that many of the patients operated on were not in good health, 'On several occasions,' says Dr. MacEwen, 'the patients were so weak that there was considerable hesitation about administering an anæsthetic. Many have been carried to the wards by their friends, as they were unable to walk except by

the aid of crutches, and that for a very short distance.' It is indeed strange that such facts are allowed to pass entirely unnoticed, and that in spite of them surgeons continue to assert that equally good results may be obtained by cleanliness, good ventilation, &c. These injuries will be generally acknowledged to be grave ones, and there is no record whatever of anything like similar results from other than aseptic methods of treatment. In reference to the statistical value of such cases, Mr. Holmes justly remarks: 'Of all wounds, perhaps those of compound fractures of the leg are the best adapted for studying the effect of different ways of dressing. Amputations and other major operations depend so much more for their success on the health of the patient and the previous course of the disease, that it is hardly possible to draw from their results any absolute conclusions as to the effect of the system of dressing. This objection applies, of course, to compound fractures to some extent (as indeed it does to every surgical affection); but much less than to amputations, especially when the severer accidents which call for immediate amputation, or which prove fatal from other injuries, are left out of account.'

Professor Volkmann has published a detailed account of all the compound fractures treated by him conservatively since he introduced the aseptic method. These are 73 in number (75 fractures), and of these 73 patients not one died.[1] In one or two cases of injury to joints, primary resection was practised. These, of course, still remained compound fractures, and are very properly included here. The following is Volkmann's list:

Part injured	No. of cases	No. treated conservatively	Secondary resection	Secondary amputation	Death
Femur . .	1	1	—	—	—
Patella . .	3	3	—	—	—
Leg . . .	43	36	2	5	—
Humerus . .	8	8	—	—	—
Fore-arm . .	20	14	3	3	—
	75	62	5	8	0

[1] In his address at the recent meeting of the International Medical Congress in London, Volkmann said: 'When I adopted the antiseptic treatment of wounds my last 12 patients, with compound fracture of the leg, had all died of pyæmia or septicæmia. From that time up to the present day I have

Now, in considering these facts we have first to notice, that no less than 48 patients were injured by direct violence, and that in 20 cases the bones were extensively shattered. A number of the cases were machine accidents, and Volkmann says with regard to these, that it was at first a very difficult matter to decide whether any attempt ought to be made to save these limbs. In 19 of the cases joints were involved in the injuries: of these 5 were resected and 3 amputated secondarily, while 11 were treated conservatively to the end. In 2 cases primary resection of the joint was performed, once at the shoulder-joint and once at the elbow-joint.

The following are the ages at which these accidents occurred:

1–10	11–20	21–30	31–40	41–50	51–60	61–70
5	13	14	17	10	9	5

With regard to the cases of secondary amputation and resection, Volkmann expressly states that they chiefly occurred soon after the treatment was first introduced, and that with increasing experience of the aseptic method these cases have become more and more infrequent; and he states that 3 cases of secondary amputation would have been unnecessary if the injuries had been, in the first instance, properly diagnosed and treated. In speaking of the results of his cases he rightly divides them into two sets, the first being those which occurred during the first year, while they were learning the method; and the second, those treated during the remainder of the period (three years or more) after they had learned the method. In the first set they had always suppuration and sometimes gangrene of portions of the skin; here they injected a strong solution of chloride of zinc at first, and afterwards they washed out the wound daily with carbolic lotion. When they found that chloride of zinc ought not to be used, and that it was unnecessary and hurtful to wash out the wound, they got very different results. Then they found, and this was the case in

treated, one after another, 135 compound fractures, and not a single patient has succumbed to either of those accidental wound diseases; 133 were cured, two died, one of fat embolism of the lungs during the first few hours, and one, a drunkard, of delirium tremens.'—*Lancet*, August 13, 1881.

about the last 60 cases, that the fracture and wound behaved as if they were subcutaneous, there was no suppuration nor gangrene of the tissues, but, on the contrary, rapid healing. As a rule, also, necrosis did not occur, though in one or two cases small detached fragments, which had been left at first, were picked out of the wound afterwards. Volkmann describes the course of the cases as follows: 'Gleich von Anfang an tritt keine örtliche Reaction, keine Jauchung, kein Wund-reinigungs-stadium auf; das Markgewebe verjaucht nicht in der Umgebung der Bruchspalte; die geöffnete Markhöhle und das blossgelegte spongiöse Gewebe werden sofort wieder durch Blutcoagula verdeckt und abgeschlossen, die so lange liegen bleiben, bis sie durch organisirte Gewebswucherungen ersetzt sind. Es entstehen keine fissuralen (Bruchspalten-) Eiterungen, keine inter-musculären, subperiostealen und parossalen Phlegmonen, keine Eitersenkungen und Eiter-retentionen, keine grösseren Necrosen. Die antiseptische Methode gestattet die Heilung der complicirten Fracturen gewissermassen unter dem feuchten Schorf.' In only three cases did Volkmann fail to eradicate the causes of fermentation.

Max Schede has also treated a number of compound fractures aseptically. There were 37 cases admitted soon after the infliction of the injuries, and only 1 did badly, and that case is said to have suffered in the first few days from fatty embolism and delirium tremens. No patient died. In 4 cases intermediate amputation was necessary, thrice on account of gangrene due to the direct violence of the injury, and once on account of gangrene in the case just alluded to, where the patient moved the limb violently and tore the posterior tibial artery, the anterior tibial having been destroyed by the injury. All these cases were the result of direct violence.

Taken along with Volkmann's 73 cases we have a total of 110 cases treated, in the first instance conservatively, without a death. Of these, 12 required secondary amputation and 5 secondary excision, leaving 93 which recovered without further interference.

Max Schede adds the following interesting facts:—Seven cases came under treatment between the second and the sixteenth day after the injury; these cannot of course be in-

cluded in the same category as those which came under treatment within a few hours after the accident. Of these, 2 died—1 admitted on the ninth day with tetanus and 1 admitted on the sixteenth day with extensive suppuration; 1 underwent secondary amputation, and 4 were treated conservatively. As I have previously pointed out, these cases illustrate the result of treatment by antiseptics, because it is hardly possible to render such a wound aseptic so long after the injury.

Before leaving Volkmann's results I shall refer to the compound fractures made by himself. He has produced 71 compound fractures on 59 patients. One of these cases died. This occurred in the case of a bleeder who died of anæmia about twenty-four hours after the operation—a case of excision of the knee for anchylosis. Leaving out of consideration, then, according to Mr. Holmes' rule, this case which died within forty-eight hours, we have 58 patients on whom compound fractures were produced without any fatal result. After the 70 compound fractures which we have to consider, secondary amputation was only necessary twice; in 1 case a central enchondroma in the tibia was found to be the cause of the deformity, and the limb was therefore amputated; in the other case there was very severe genu valgum in consequence of arthritis deformans, but the exact reason for amputation is not given. As to the bones affected, we have mention of 16 compound fractures produced on the femur and 45 on the bones of the leg. The other operations were for ununited or badly united fractures, but the bones affected are not mentioned.

In comparison with these results Volkmann says, that the journals of the surgical wards at Halle for former years contain notes of 11 compound fractures of the thigh, of which 6 died, and of 64 compound fractures of the leg, of which 26 died. The proportion of cases which required secondary amputation is not mentioned, but it is stated that many of the injuries were very simple and the result of indirect violence.

During the year 1878–79 Bardenheuer, operating aseptically, made 28 compound fractures on healthy bones without bad result. He also performed 53 resections—15 of the hip and 12 of the knee-joints—without a death.

MacCormac gives the results of the aseptic practice in

St. Thomas's Hospital. There were 16 cases of compound fracture treated (2 of the femur, 3 of the upper extremity, and 11 of the tibia) without a death, and in only one instance was secondary amputation necessary. He contrasts these results with those obtained in the former six years during which 54 cases of compound fracture were not treated aseptically, and of these 12 died (4 from pyæmia and 4 from erysipelas); one case required secondary amputation on account of gangrene; and in several there was suppuration or erysipelas and a protracted recovery.

MacCormac also referred to 45 compound fractures made by the surgeon (since the address the number has risen to 57), without a death. Thirty of these involved the knee-joint, and though the wound in the soft parts was comparatively small, yet it was 'quite large enough in many instances to allow serious inflammation and putrefactive changes to take place in it.' In no single instance did any serious result follow.

If, now, we sum up the facts already obtained and, following Mr. Holmes' suggestion, exclude all cases of primary amputation, and of death within forty-eight hours after the injury, we get the following remarkable results:—

I. Accidental Compound Fractures.

Adding together the results of Lister, Volkmann, Schede, and MacCormac, we find that there were 164 cases of compound fracture treated conservatively, in which an attempt was made to purify the wound,[1] and of these 2 died, giving a mortality, after accidental compound fractures, of 1·2 per cent. But then one death certainly, and the other probably, was independent of the injury. Among these are included a few cases of compound fracture of the skull, but if we take limbs alone, we find that 158 limbs were affected with compound fracture, and among these secondary amputation was performed in 15 cases and secondary excision in 5, leaving 138 limbs which were cured without operative interference.

[1] The numbers would be larger now, see note on p. 506.

II. Intentional Compound Fractures.

Adding together the results of Lister, Volkmann, MacEwen, Bardenheuer and MacCormac, we have 530 patients affected with 1,072 compound fractures, and of these only 3 died, giving a mortality of ·56 per cent., the causes of death being in each case quite independent of the operation. Looking at the results to the limbs, we find that 766 limbs had 1,068 compound fractures, and that among these secondary amputation was only necessary in 4 instances, or ·51 per cent.

Adding together the cases of accidental and intentional compound fractures, we find *that* 694 *patients were affected with* 1,239 *compound fractures treated aseptically, and of these* 5, *or* ·72 *per cent., died.*

Or looking at the results as regards the limbs, we find *that* 924 *limbs were affected with* 1,226 *compound fractures, and of these* 902 *recovered without further operation, while* 19 *required secondary amputation and* 5 *required secondary excision.*

In contrast with these results we have the following facts :

Volkmann and Fränckel [1] found a record of 885 compound fractures of the leg in the reports of the German and English hospitals, and, of these, 339, or 38·5 per cent., died. In the above 694 cases treated aseptically, I have not been able to give the exact numbers as regards the bones affected, but by far the greatest number of bones injured were those of the lower extremity, the injuries of the thigh and leg being about equal in frequency, the femur being perhaps most often affected. However, as the mortality after compound fracture of the femur is greater under ordinary circumstances than the mortality after compound fracture of the bones of the leg, the comparative value of these statistics is increased.

Baum in Göttingen [2] lost	38	p. c. after compound fracture of the leg.
Billroth in Zürich [2]	38·7	,, ,, ,,
In Breslau [2]	40·5	,, ,, ,,
In Halle [2]	40·6	,, ,, ,,
In Bonn [2]	41·8	,, ,, ,,

[1] Volkmann's *Sammlung*, 117 118. [2] *Ibid.*

Mr. Holmes[1] states that in St. George's Hospital, from 1865 till 1878 inclusive, after excluding cases of primary amputation and deaths within forty-eight hours, there were treated by the ordinary methods 162 cases of compound fracture of the leg, of which 40, or 24·6 per cent., died. A considerable proportion of these cases was treated during a period in which cleanliness, good ventilation, &c., were greatly in vogue, and by surgeons who were much impressed with their necessity. Among these 40 deaths were 21 from pyæmia alone, and several of the remaining deaths were due to other septic diseases.

I have previously, at p. 510, given the results in St. Thomas's Hospital, the mortality there being 22·2 per cent.

These records correspond with the expressed views of surgeons of all ages as to the dangers of compound fractures produced accidentally or of operations in which compound fractures are made. On the other hand we have also the generally acknowledged fact that *subcutaneous* osteotomies are practically free from danger, and the subcutaneous method is, as we have previously seen, a form of aseptic treatment. The foregoing facts show that it is not necessary to have recourse to subcutaneous operations to ensure safety, but that the Listerian method is equally, if not more efficacious, while the free access to the bone is in many cases a great advantage, indeed in some an absolute necessity.

I must now again refer to Carl Reyher's results in the Russo-Turkish war (see p. 401, *et seq.*) During this war he treated 22 cases of compound fracture of the extremities aseptically (by 'primary antiseptics,' as he terms it), and of these 4 or 18 p.c. died, secondary amputation being only necessary in one instance. The following list shows the cases and the results:—

Part injured	No. of cases	Died	Secondary amputation	Healed with retention of limb
Humerus . . .	4	—	—	4
Fore-arm . . .	3	1	—	2
Thigh . . .	3	3	1	—
Leg	12	—	—	12
Total .	22	4	1	18*

*Or 81·8 p. c.

[1] *St. George's Hospital Reports*, vol. ix.

During the same time he had 62 fractures of the long bones treated with antiseptics or, as he calls it, with 'secondary antiseptics,' and of these 23, or 35·3 p.c., died. The following is the table :

Part injured	No. of cases	Died	Secondary amputation	Healed with retention of limb
Humerus . . .	12	5	3	5
Fore-arm . . .	3	—	—	3
Thigh . . .	25	13	5	12
Leg	22	5	2	17
Total .	62	23	10	37*

*Or 69·6 p. c.

He also refers to 27 similar fractures not treated antiseptically at all, but he gives no details of them, and only states that up to the time of writing 8 had died of pyæmia.

The following are the fatal cases where aseptic treatment was attempted :—

One compound fracture of the thigh died of fatty embolism ; it is not mentioned how long after the injury the patient died : in this case the femur was completely shattered throughout its whole extent, the wound of entrance being near the pubis. One compound fracture of the thigh died from exhaustion, the result of profuse suppuration. One compound fracture of the thigh died from septic suppuration. One compound fracture of the forearm—a shell injury—died of pyæmia.

With regard to these cases I need merely point out the extreme difficulty of thoroughly disinfecting the wound and of removing all portions of clothing from the wound. Reyher refers to this point, and it seems that in 3 of the 4 fatal cases in which the aseptic method was tried, disinfection was not successful. This is of course only what might be expected. There is nothing magical about the injection of an antiseptic into wounds, and unless the fluid reaches all the recesses of the wound, and destroys all the causes of fermentation which have been introduced into it, the wound cannot be expected to follow an aseptic course ; it is not an aseptic but a septic wound, and the consequences of such a wound will be those of a septic not of an aseptic one.

In considering the results of other forms of antiseptic treatment, the only detailed account to which I can refer is Krönlein's report of the results of the open method in Zürich (see p. 410). Krönlein states that between 1860 and 1867, 160 cases of compound fracture were treated, and of these 67 died; 86 of them being treated conservatively with 21 deaths. Between 1867 and 1871, 102 compound fractures were treated, of which 27 died; 65 of these being treated conservatively with 14 deaths.

Though Krönlein tells us the number of cases amputated, and the number treated conservatively to the end, he does not tell us the number of primary and secondary amputations. Now, of course, in considering the results of compound fractures, we must divide them into those amputated primarily, and those in which conservative treatment was tried; the latter group being subdivided into those which required secondary amputation and those which were treated conservatively to the end. Of course the cases treated conservatively to the end are the most favourable class, because they did not require secondary operation. But in judging of the success of conservative treatment, it is necessary to know in what proportion of cases that treatment failed and secondary operation was required. Now it is only of the former—the successful cases—that Krönlein gives us information. With the view of getting a correct impression on this point, I have gone over his statements, and the following are the facts, so far as I have been able to gather them.

Of the 160 cases treated between 1860–67, 38 were amputated primarily, with 19 deaths; 21 were amputated secondarily with 17 deaths; and 15 were amputated, but whether primarily or secondarily, I have not been able to ascertain; of these 9 died. 86 were treated to the end conservatively with 22 deaths. Now according to our rule the primary cases are omitted. As to the 15 amputations with 9 deaths, I don't know what they are, and therefore we shall put them in a group by themselves. This leaves us with 107 cases in which we are certain that conservative treatment was attempted, and of these 39, or 30·4 per cent., died. The result then, so far as we know it, is—

No. of cases	Deaths	Secondary amputation	Recovered without mutilation
107	39	21	64
(certain)		(certain)	(or 59·8 p. c.)

Looking at the results of the open method from 1867 to 1871, we find that 102 cases in all were admitted, and of these 27 died. Of these 19 were amputated primarily with 5 deaths; 9 were amputated secondarily with 4 deaths; 9 were amputated, but whether primarily or secondarily I do not know, with 4 deaths; 65 were treated conservatively to the end with 14 deaths. Treating these cases like the former, we may say that 74 cases were certainly treated conservatively, and of these 18, or 24·3 per cent., died. Thus—

No. of cases	Deaths	Secondary amputation	Recovered without mutilation
74	18	9	51
(certain)		(certain)	(or 68·9 p. c.)

It is to be remembered in both cases that some of the doubtful amputations were probably secondary, so that these results are the most favourable. I may give further details of the cases which we know about according to the limbs affected.

Cases treated Conservatively.

(1860–67.)

Part affected	No. of cases	Percentage of cases treated conservatively	Deaths	Secondary amputations	Recovered without mutilation
Femur . .	8	61·5	3	1	5
Leg . .	77	81	26	15	49
Humerus .	9	47·3	4	2	4
Fore-arm .	13	39·3	6	3	6
Total .	107	66·8	39	21	64

Cases treated Conservatively.

(Open Method 1867–71.)

Part affected	No. of cases	Percentage of cases treated conservatively	Deaths	Secondary amputation	Recovered without mutilation
Femur . .	12	70·5	3	1	9
Leg . . .	38	84·4	12	7	22
Humerus . .	14	51·3	2	1	11
Fore-arm . .	10	62·5	1	—	9
Total .	74	69·1	18	9	51

It will thus be seen that some improvement followed the introduction of the open method, but this in no way corresponds to that effected by the aseptic method.

The causes of death are not given, but of the 39 deaths of the first period, at least 27 were from pyæmia and septicæmia (14 of the cases treated to the end conservatively and 13 of the secondary amputations). Of the 18 deaths of the second period at least 8 were from pyæmia and septicæmia (5 of the cases treated conservatively and 3 of those amputated secondarily). As I have said, this is the most favourable statement possible, and it does not include those which died of other septic diseases.

I do not find any facts of statistical value, with regard to the results with other forms of antiseptic treatment, but the good results of irrigation and the water bath and of crust formation are well known, and have already been alluded to in the history of the subject. The whole tendency of the facts published in recent times is, however, to show that success increases according as the method adopted fulfils more and more the requirements of the aseptic principle; and the same is evident if we look at the history of the subject and see the successes obtained by the use of balsams, of crust formation, of irrigation and the water bath, and of subcutaneous surgery.

CHAPTER XX.

RESULTS OF ANTISEPTIC SURGERY—(*continued*).

Abscesses connected with disease of the vertebræ. Best situation for opening psoas abscesses : best time for opening them : after-treatment and after-progress. Table of Mr. Lister's results : general summary and remarks on these cases. Comparative statistics are wanting. Sir James Paget's views.

Before discussing these results, I think that it will be most convenient to consider the last group of cases to which I intend to refer, viz. cases of abscesses connected with disease of the vertebræ. The cases which I give here were treated by Mr. Lister between the end of 1871 and 1879 ; and I will mention all the cases which occurred during that period. I have been careful to take only those abscesses which were unmistakably connected with disease of the vertebræ, as indicated by curvature, by the history of the case and the symptoms present, and, in several instances, by the presence of pieces of bone in the pus. Of course this list, like the others, does not represent all the cases which Mr. Lister has ever treated aseptically, for he had several cases under his care before this period, and both before and during it, he has treated a number of similar abscesses in private practice. Here, as in other instances, his results have been better in private practice than in hospital, because the cases were attended to either by himself or by skilled assistants, while in hospital it was often necessary to leave the changing of the dressings to students.

In the treatment of these abscesses the general principles of aseptic surgery are carried out in the manner before described, and I need not recapitulate the points here. I must, however, say a few words as to the best situation for opening psoas abscesses. An abscess which has passed into the thigh,

and which is pointing in the neighbourhood of the lesser trochanter, is usually, under the ordinary modes of treatment, opened at its most prominent situation at the upper and inner part of the thigh. Now if the aseptic method is adopted, it is quite evident that an incision in that situation leaves very little space, only two or three inches, for the overlapping of the dressings, while the edge of the dressing, being near the anus and genito-urinary organs, is very liable to get soiled with the excretions. Case No. 1 is an example of this, and hence it is well, if possible, to avoid this risk; indeed, these abscesses are now never opened in that situation. Almost all the psoas abseesses have up till the present time been opened above Poupart's ligament, and the results have been very satisfactory. An incision is made near the anterior superior spine of the ilium, much in the line for tying the external iliac artery, and the muscles are carefully divided as in that operation. When the transversalis fascia is reached, the pus can generally be made to bulge beneath it, and then a pair of dressing forceps are pushed into the abscess cavity, the opening dilated, all the pus squeezed out, and a large-sized drainage-tube introduced. Though this opening is not dependent, yet, if the tube be large enough, the drainage is perfectly satisfactory, and there is no accumulation of discharge. The cavity in the thigh drains upwards into the iliac fossa and closes entirely in a few days. The after-treatment has been already described; after a time, in these eases, a drainage-tube without holes except at its upper extremity, is most useful.

Mr. Chiene advocates the opening of these abscesses above the crest of the ilium behind by means of an incision at the outer border of the quadratus lumborum. The advantages which he claims for this method are the following: the orifice is dependent, and thus the drainage is most satisfactory; the incision is made into the upper part of the abscess, and thus the whole of the cavity in the iliac fossa closes rapidly by adhesion of the granulations, so that, after a few days, there is only a short sinus leading directly to the seat of disease. This situation is an advantage, more especially in the after-progress of the case, when the sinus becomes too narrow to admit a drainage-tube; for if a sinus is long and tortuous, the drainage

of the discharge from the seat of disease is of course more likely to be imperfect than if the sinus is short and leads directly to the diseased bone; further, the orifice of the drainage-tube is still more distant from sources of putrefaction in this situation than when the incision is in front; the dressing can also be very securely applied and fixed.

These abscesses ought to be opened as soon as they are detected. The spontaneous absorption of abscesses is a very rare occurrence, and the treatment by repeated aspiration is very apt to be unsatisfactory. The earlier the abscess is opened, provided that it is done aseptically, the better, and this for two chief reasons. In the first place the longer the pus is left, the larger does the abscess cavity become; and thus for the first few hours after it is opened there will be, corresponding to the extent of the abscess, a proportionate amount of serous oozing, which, if great, will severely tax the efficiency of the gauze dressing; there will also of course be a larger cavity to heal. In the second place the pus being pent up in a cavity, tends, by tension and consequent nervous disturbance, to keep up and even aggravate the chronic inflammation of the bony tissue, and therefore, so long as the pus is there, the chronic inflammation is kept up and the disease of the vertebræ progresses instead of improving. On the other hand, when the pus is let out, and care is taken that causes of fermentation are not admitted, a great source of disturbance is removed, and an opportunity is afforded for the cessation of the disease. For these reasons, therefore, the abscess ought to be opened as soon as possible. I do not of course mean to advise the early opening of these abscesses where the aseptic method is not to be used, or where the surgeon has not had sufficient experience of that method to be able to rely with considerable certainty on excluding the causes of fermentation. But if the surgeon considers that he can exclude these causes, then I believe that the best thing for the patient is to open the abscess as soon as possible.

The after-progress of these cases is exceedingly satisfactory. For some hours after the abscess is opened, there is a profuse discharge of bloody serum, due no doubt to the effect of the diminution of pressure on the vessels in the granulation tissue

lining the abscess cavity. This discharge rapidly diminishes, and in a few days becomes very slight. After the original pus is evacuated there is no more pus formation from the abscess cavity, provided that the drainage is efficient and that the discharge is kept aseptic. There may be a little suppuration from the granulations surrounding the orifice of the drainage-tube, which are irritated by the direct application of the carbolic acid, but there is no suppuration from the deeper parts. As a consequence of the slight amount of discharge the dressings are not often changed, and generally, in a few weeks after the abscess has been opened, the case is only dressed once a week, and then not because discharge has reached the edge of the dressing, but, for the reasons given on p. 93, because it is considered that the dressing is no longer sufficiently antiseptic.[1] A sinus often remains for a long time, sometimes for months, and the weekly dressings must be continued till it finally closes, the greatest care in carrying out the minutest aseptic precautions being required till healing is complete. Never be tempted, however slight the discharge, or however apparently superficial the sore, to give up the aseptic method or to substitute boracic for carbolic dressings. During the whole treatment of the case the patient must be kept absolutely recumbent, whether lying on his back or on his side is of little consequence, and must never be allowed to raise his shoulders for any reason whatever. Generally, after the sinus has healed, it is well to maintain the perfectly recumbent position for at least six weeks longer, so as to ensure that the bones are sound before the weight of the body is allowed to tell on them. The maintenance of absolute rest is essential for success.

The constitutional state of the patient rapidly improves after the abscess is opened. If hectic fever is present beforehand, it generally rapidly subsides (see T. Chart LIV), and the temperature becomes normal, and remains so throughout ; and if the temperature is normal before the abscess is opened it does not rise nor assume a hectic type, as is so common when the

[1] I may just recall the fact alluded to on p. 89, that when dressings are left on for a week, there sometimes occurs a little irritation beneath them, and that, therefore, it is well to rub a little of the salicylic cream on the skin around the wound before applying the dressing.

discharge is allowed to putrefy. The general condition of the patient corresponds entirely to this state of the temperature: if he has been weak and suffering before, he rapidly regains strength, loses his pain, and puts on flesh; his appetite returns, and he soon feels in a state of perfect health. The reason for this is evident; the patient is not exhausted by the daily loss of a large quantity of discharge, while he is relieved from the presence of the abscess. Cod-liver oil and iron are, of course, given throughout, but stimulants are rarely necessary, at least after the first week or two. Although these cases remain so well, and although the discharge is soon practically *nil*, yet they are as a rule very tedious, and it is well not to reckon on cure in less than six or eight months; some cases indeed last much longer. It is expedient, therefore, to warn the patient's friends of this before the abscess is opened, for otherwise they may get discouraged and take the patient away; and if aseptic treatment is not continued to the end, the chances of recovery are almost as slight as if it had never been employed.

A number of examples of the 'aseptic course' of these abscesses just described will be found in the following table, so that I need not detail a case here.

PSOAS AND

No.	Name and Age	Date of Operation and Discharge; with Result	Disease.
1	Jane T., 38 . .	*Op.*, Dec. 21, 1871. *Dis.*, Jan. 31, 1872. *Result*, putrefied.	Large psoas abscess forming a bulging tumour in the thigh and extending high up in the abdomen. First noticed six months previously, pain in back, &c.
2	Donald T., 24 .	*Op.*, Feb. 19, 1872. *Dis.*, Feb. 1873. *Result*, cured.	Lumbar abscess. Patient hurt his back two-and-half years ago, pain in back, &c.
3	Thomas G., 5 .	*Op.*, April 2, 1872. *Result*, unknown, probably cured.	Psoas abscess in connection with advanced spinal disease. A lumbar abscess appeared later.
4	William L., 45	*Op.*, April 1872. *Dis*, April 3, 1873. *Result*, cured.	Lumbar abscess. Curvature of spine, &c.
5	Thomas M., C2	*Op.*, July 4, 1872. *Dis.*, April 2, 1873. *Result*, died of an independent cause.	Lumbar abscess. Patient first noticed a swelling three months previous to admission. There was no curvature but great pain on pressure over the spine in the lumbar region.
6	Peter L., 35. .	*Op.*, Sept. 1872. *Dis.*, Oct. 21, 1873. *Result*, cured.	Psoas abscess with marked curvature of the spine about the lower dorsal region. Abscess pointing in the thigh. Disease began nine years previously. Suffered great pain and had been getting weaker and thinner.

LUMBAR ABSCESSES.

Treatment	Remarks
Abscess opened in thigh and 40 oz. of pus evacuated. Drainage-tube inserted.	On December 26, it was found that the menstrual flow had soaked the dressings and the discharge had a disagreeable odour. On December 28, a large piece of bone came away. As the abscess was undoubtedly putrid, and as the patient was very anxious to go home, she was allowed to do so. At that time there was profuse discharge and the patient was getting weaker.
Opened, 10 oz. of pus evacuated. Drainage-tube inserted.	The discharge rapidly decreased in amount, being in the main serous though occasionally somewhat purulent. The abscess had completely healed on October 24, but on November 11 the cicatrix gave way and a sinus was re-established. This healed permanently about the end of January.
Opened, drainage-tube inserted. (The lumbar abscess was opened in October).	After opening the first abscess the discharge was considerable and purulent, but there was no smell. Many fragments of bone came away during the course of the summer, and in October a lumbar abscess was found and opened. The case is not completed in the note books, the last note having been taken on April 20, 1873, to the effect that there was very little discharge, and that the general health was good. I should think that the case was in all probability cured.
Abscess opened. Drainage-tube inserted.	Aseptic course. The wound was found healed on August 11, but the scar again opened. Healed again on October 13; again the scar gave way. Healed permanently on December 10. For some time the patient could not do without a spinal support, but in July 1877 he showed himself, and was then perfectly well and strong and able to do any kind of work.
Opened. Drainage-tube inserted.	On November 28, some curvature of the spine was noticed. In February the dressings were changed weekly, there being only a small sinus, which at the end of March had almost absolutely healed. At this time a small glandular abscess formed in the neck and was opened and dressed with boracic lint. On March 23, erysipelas attacked this wound and quickly spread over the head and neck. The patient got rapidly weaker, and died on April 2. (This is really a case of cured lumbar abscess dying of another cause before leaving hospital.)
Opened. Drainage-tube inserted. The material evacuated was very thick.	Aseptic course, the discharge being purely serous and very slight in quantity. Healed in September 1873.

PSOAS AND

No.	Name and Age	Date of Operation and Discharge; with Result	Disease
7	T. H., 9 . . .	*Op.*, Feb. 8, 1873. *Dis.*, March 8, 1873. *Result*, in process of cure.	Three lumbar abscesses. Curvature in the dorsal region. Patient weak and thin.
8	Helen T., 24 .	*Op.*, May 7, 1873. *Dis.*, Dec. 22, ,, *Result*, cured.	Psoas abscess, which had passed into the thigh. Patient began to feel weakness in the back about six months previously. She had noticed the swelling in her thigh for three months.
9	Donald R., 26 .	*Op.*, June 2, 1873. *Dis.*, Jan. 23, 1874. *Result*, cured.	Psoas abscess pointing in the thigh. Curvature in the lower lumbar region.
10	Jane R., 27 . .	*Op.*, July 2, 1873. *Dis.*, Feb. 2, 1876. *Result*, cured.	Patient was admitted in June 1873 on account of suppuration of the sheaths of the flexor tendons of the hand following an incision into a bursitis of these tendons not performed aseptically. On July 2, a psoas abscess was found in the right iliac fossa. Had suffered from pain in her back for about a year, and for some time from abdominal tenderness.
11	Anne McK., 22	*Op.*, Aug. 21, 1873. *Dis.*, Spring 1875. *Resu t*, cured.*	Lumbar abscess with well-marked curvature of the lower dorsal vertebræ. A second abscess formed later.
12	— W., 32 . .	*Op.*, Sept. 2, 1873. *Died*, Feb. 3, 1874. The cause of death was phthisis.	Psoas abscess pointing in the thigh. Curvature in the lumbar region; great pain in the back. Advanced phthisis.
13	Eliza M., 30 .	*Op.*, Feb. 19. 1874. *Dis.*, Nov. 10, ,, *Result*, in process of cure.	Dorsal abscess. Great tenderness on pressure over the whole dorsal vertebræ and curvature of the middle ones. Patient very weak and anæmic. The pain began a year ago. Patient half-witted.

LUMBAR ABSCESSES (*continued*).

Treatment	Remarks
Opened. Drainage-tubes inserted.	On Feb. 23, the two smaller abscesses had closed. The drainage-tube was removed from the other, and by March 8, the discharge was very slight. The parents were very anxious to take the child home, and they were allowed to do so after having been instructed in the method of dressing. The general health was at that time improving.
Opened. Drainage-tube inserted, 30 oz. of pus were evacuated.	Aseptic course. After June 3, the case was dressed every second day, and after July 6, every three days. Healed about the beginning of November.
Opened above Poupart's ligament. Drainage-tube inserted, 23 oz. of pus evacuated.	Aseptic course. No more pus. Found healed on October 10. Patient allowed to get up for the first time on December 6.
Abscess opened above Poupart's ligament, 20 oz. of pus containing pieces of bone were evacuated. Drainage-tube was inserted.	The discharge rapidly diminished, and the case was soon dressed only once a week. Aseptic course, the discharge being purely serous. On December 2, 1874, abscesses were found in connection with the elbow-joint and opened, the patient's health being then good and the psoas abscess almost healed. When the patient left hospital everything was sound. When heard of in 1877, she was well and strong and had a fairly movable elbow-joint.
Opened, 10 oz. of pus evacuated. Drainage-tube inserted.	Aseptic course. Quite healed on July 31, 1874. On September 15, 1874, she was allowed to sit up for the first time, but afterwards felt great pain in the dorsal vertebræ. She again sat up on October 3, but again had pain. On October 22, a small abscess was opened at the side of the former one. This healed during the spring of 1875, and the patient was discharged cured. (The exact dates of healing and discharge cannot be ascertained.)
Opened, 24 oz. of pus evacuated. Drainage-tube inserted.	Till the middle of November the case went on well, and the general health seemed to improve, but from that time the lung disease rapidly got worse and the patient gradually became weaker. Diarrhœa set in in January. During the last two or three weeks there was a considerable increase in the amount of the discharge, which became purulent.
Opened, 24 oz. of pus evacuated. Drainage-tube inserted.	The abscess went on as usual, the discharge diminishing, and the patient's health improving when, on April 9, she tore off the dressings. Next day there was a slight smell, and the wound was injected with 1-20 carbolic lotion. The discharge thereafter was odourless. No notes are given from the end of April till November 10, when she is said to have been 'discharged in process of cure.' (See T. Chart LV.)

PSOAS AND

No.	Name and Age	Date of Operation and Discharge; with Result	Disease
14	Margaret M., 7	*Op.*, July 1874. *Dis.*, Jan. 1875. *Result*, cured.	Psoas abscess, angular curvature in the lumbar region. Began six months before admission.
15	Margaret S., 20	*Op.*, Aug. 3, 1874. *Dis.*, Sept. 2, 1875. *Result*, cured.	Dorsal abscess. Angular curvature in the dorsal region. Symptoms began eighteen months before admission. Patient thin, without appetite, and had cough and hæmoptysis.
16	Susan J., 12 .	*Op.*, Aug. 17, 1874. *Dis.*, April 17, 1875. *Result*, putrefied.	Psoas abscess. Curvature in the lumbar region.
17	William M., 6 .	*Op.*, Sept. 14, 1874. *Dis.*, Nov. 7, 1876. *Result*, cured.	Psoas abscess, which had not yet passed into the thigh. Curvature in the lower dorsal region.
18	William L., 25	*Op.*, Jan. 19, 1875. *Dis.*, June 19, „ *Result*, left hospital before healing was complete.	Psoas abscess. Curvature in the lumbar region.
19	Thomas M., 15	*Op.*, July 4, 1875. *Result*, cured.	Lumbar abscess. Patient was admitted in May with an abscess connected with a carious rib.
20	Mary C., 4 . .	*Op.*, Nov. 18, 1875. *Dis.*, June 27, 1876. *Result*, putrefied.	Psoas abscess with disease of the vertebræ.
21	Margaret W., 24	*Op.*, Nov. 16, 1875. *Died*, March 17, 1877. *Result*, died of exhaustion.	Psoas abscess pointing in the thigh. Acute curvature in the lumbar region. The abscess extended all round the thigh.

LUMBAR ABSCESSES (*continued*).

Treatment	Remarks
Opened, 14 oz. of pus evacuated. Pieces of bone in the pus. Drainage-tube inserted.	Aseptic course. Healed November 20, 1874. Allowed to sit up on January 9, 1875. Exact date of discharge not given.
Opened, 5 oz. of pus evacuated. Drainage-tube inserted.	Aseptic course. Healed in August 1875. Patient rapidly picked up flesh and regained her appetite after the abscess was opened. When she was discharged she was stout and well.
Opened, 8 oz. of pus evacuated. No drainage-tube was inserted.	On September 8, this wound had healed, but a collection soon reformed, which, on evacuation, was found to consist of clear yellow serum. During the month of February the case seems to have putrefied, and on March 22, aseptic measures were stopped and wet boracic lint applied. Sent home.
Opened by a dissection as if to tie the external iliac artery. Drainage-tube inserted.	Aseptic course, but the patient was extremely restless, and it was very difficult to prevent him from sitting up. To this is possibly due the long delay in healing. This abscess was found healed on September 15, 1876. A second abscess formed on the opposite side and was opened during October 1875. This had healed before the patient was discharged.
Opened like No. 17, 15 oz. of pus evacuated.	The wound was doing well, but the patient got tired of hospital, and would not stay longer. The tube was therefore removed and he was taught how to dress the wound aseptically. When he left his urine was albuminous, but its state on admission is not given.
Opened. The spinous process of one of the lumbar vertebræ was felt to be bare. Drainage-tube inserted.	This case seems to have followed an aseptic course; the patient left the hospital healed and cured. But the notes are abominably taken, and there is no note either of the date of healing or of the date of dismissal.
Opened like No. 17, above Poupart's ligament. Drainage-tube inserted.	This case did wel till the Christmas holidays, when putrefaction seems to have occurred. After that time there was profuse purulent discharge, though for the most part odourless. The child was getting thinner and weaker, and the parents wished to take her home.
Opened, 39 oz. of pus evacuated containing pieces of bone.	All through the case there was great difficulty in getting free drainage, and consequently there was an unusual amount of discharge, the case requiring dressing about every second day. Latterly the patient suffered severe pain in the thigh, leg, and back, with constant vomiting. On post-mortem examination the lumbar vertebræ were found to be completely disorganised, cavities containing cheesy material being found in the bodies, transverse and spinous processes of the vertebræ. The bodies of two vertebræ were absent. The liver, spleen, kidneys, and intestines were in a state of waxy degeneration. For T. Chart just before death, see No. LIII.

PSOAS AND

No.	Name and Age	Date of Operation and Discharge; with Result	Disease
22	Ellen S., 32 . .	*Op.*, Nov. 14, 1875. *Dis.*, Feb. 3, 1876. *Result*, cured.	Large lumbar abscess. Curvature of the spine.
23	John D., 18 . .	*Op.*, March 22, 1876. *Dis.*, March, 1878. *Result*, cured.	Psoas abscess pointing in the left groin. Curvature of the spine. Patient had been ill for a year. He was in an extremely weak and emaciated state. At a later period an abscess formed in the right thigh.
24	John D., 24 . .	*Op.*, April 4, 1876. *Died*, Oct. 2, „ The cause of death was phthisis.	Psoas abscess. Curvature of the spine in the lumbar region. Patient was very weak, and had advanced phthisis.
25	Eliza T., 19 . .	*Op.*, Aug. 1, 1876. *Dis.*, July 1880. *Result*, cured.	Admitted with a psoas abscess on the left side, which had not yet reached the thigh. Great tenderness and curvature of the spine in the lumbar region. At a later period a psoas abscess formed on the other side and also a lumbar abscess.
26	Helen E., 50 .	*Op.*, Nov. 16, 1876. *Dis.*, Sept. 4, 1877. *Result*, cured.	Patient injured her back a year before admission. It had been very weak and painful ever since, and when admitted there was a psoas abscess pointing in the thigh. Patient was very weak.

LUMBAR ABSCESSES (*continued*).

Treatment	Remarks
Opened. Drainage-tube inserted.	Aseptic course. Healed November 20, 1876. (See T. Chart LVI.)
Abscess opened above Poupart's ligament. Drainage-tube inserted.	The psoas abscess went on very well. A large abscess formed in the other thigh, and was opened on February 11. The patient was removed to a private house in October 1877, and placed under Dr. Bishop's care. Dr. Bishop writes as follows in the *British Medical Journal* for January 31, 1880: 'John D., healed in March 1878. Left Edinburgh in May. When last heard of he was able to walk without support, and was feeling quite strong.' (For T. Chart for some days after the second abscess was opened, see No. LIV.)
Opened above Poupart's ligament. Pieces of bone escaped with the pus. Drainage-tube inserted.	The discharge seems to have remained aseptic, though latterly it increased somewhat in amount, necessitating a change of dressings every two or three days. His cough got worse, he became weaker, and died of phthisis.
Opened above Poupart's ligament, 20 oz of pus evacuated. Drainage-tube inserted.	Aseptic course, but the case was extremely tedious. A psoas abscess formed on the right side, and was opened on January 16, 1878. In March, after a gymnastic performance in her bed, she felt great pain in her back, and a lumbar abscess formed and was opened on April 17, 1878; bare bone was felt by the finger. The case went on with almost no discharge, the dressings being changed only once a week, till August 1879, when the first sinus healed. In the beginning of October this opened up again. On November 22 the right psoas abscess was found healed, and on December 27, the lumbar abscess had healed. Although the original sinus had not yet healed, the patient was allowed to get up in February 1880. This sinus healed in June 1880, and the patient went home in the beginning of July. During the whole time the patient had been in good health, and her rosy cheeks (not hectic) and appearance of perfect health rendered it difficult to persuade any one that she was suffering from disease of the spine with two psoas and one lumbar abscess. (See T. Chart LVII. taken at the time when the second abscess was opened.)
Abscess opened above Poupart's ligament, $18\frac{1}{2}$ oz. of pus evacuated. Drainage-tube inserted.	Aseptic course. Healed July 5, 1877. Before her discharge she was able to walk about. (See T. Chart LVIII.)

PSOAS AND

No.	Name and Age	Date of Operation and Discharge; with result	Disease
27	John R., 10 . .	*Op.*, Nov. 20, 1876. *Dis.*, Aug. 5, 1877. *Result*, cured.	Psoas abscess pointing in the thigh. Disease of the spine.
28	Hugh McL., 6.	*Op.*, Jan. 29, 1877. *Dis.*, Aug. 1878. *Resu t*, improving.	Psoas abscess on the left side pointing in the thigh. Extensive disease of the vertebræ and curvature of the lower dorsal and upper lumbar vertebræ. An abscess formed on the other side at a later period.
29	Michael C., 21.	*Op.*, Jan. 28, 1877. *Dis.*, May 1878. *Result*, cured.	Lumbar abscess on the right side. Pain in the back. A second lumbar abscess formed.
30	John B., 25 . .	*Op.*, Feb. 11, 1877. *Dis.*, Feb. 1878. *Result*, cured.	Psoas abscess which had not passed into the thigh. Had been treated for spinal disease for eighteen months. Patient very weak.
31	John D., 29 . .	*Op.*, Oct. 12, 1877. *Dis.*, Jan. 1878. *Result*, died.	Psoas abscess which had passed into the thigh. The curvature of the spine began at the twelfth dorsal and the most prominent part was at the second lumbar vertebræ. Patient much emaciated and very weak.

Lumbar Abscesses (*continued*).

Treatment	Remarks
Opened in the thigh. A sound was then passed under Poupart's ligament, the point projected against the skin and cut out. The lower wound was then stitched. Drainage-tube inserted into the upper.	Aseptic course, soundly healed June 1877. The wound in the thigh healed in a few days, but pus reaccumulated there, and it was necessary to open the scar. (The object of the procedure was to obtain an opening above Poupart's ligament as far away from the pubis as possible without the difficulty of a special dissection. It was hoped that the wound in the thigh would heal by first intention, and that the part of the abscess in the thigh would drain into the abdomen.) (See T. Chart LIX.)
Opened like No. 27. Very thick pus containing pieces of bone was evacuated.	For some time the case went on very well, but it was a matter of extreme difficulty to keep on the dressings owing to his deformity. An abscess formed on the opposite side and was opened in May 1877. Putrefaction occurred during the summer. Of this patient Dr. Bishop writes: 'His father removed him in August 1878 to the West of Scotland. He was then considerably improved, having youth on his side to resist the septic influences; and when he was last heard of he was running about.'
Opened. Drainage-tube inserted.	Aseptic course. The second abscess was opened February 25, 1877. Dr. Bishop says, 'Michael C. healed in March 1878. Left in May. In July 1878 he was able to go to Peterhead to the herring fishing. He returned to the fishing in 1879.' (See T. Chart LX.)
Abscess opened above Poupart's ligament. Drainage-tube inserted.	Aseptic course. Dr. Bishop says: 'John B., healed in December 1877; left in February quite strong and well. When last heard of, he was employed as a colporteur.' (See T. Chart LXI.)
Opened, 35 oz. of pus evacuated. Drainage-tube inserted.	The patient apparently suffered from carbolic poisoning, and therefore the dressings were left on for a long time and were made very small, and creosote and ultimately thymol were substituted for carbolic acid. As the result of these changes, putrefaction occurred. The discharge became foul and exceedingly profuse and the patient became rapidly weaker. As he expressed a desire to go home, he was allowed to do so. He died about a month after he got home.

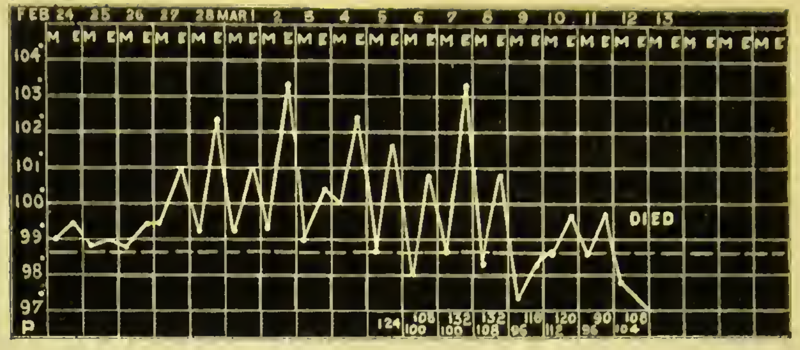
PSOAS AND

No.	Name and Age	Date of Operation and Discharge; with Result	Disease
32	Lucy S., 22 . .	*Op.*, July 23, 1878. *Dis.*, June 1879. *Result*, cured.	Large psoas abscess. Also a collection of fluid in the gluteal region. Curvature of the lower dorsal vertebræ.
33	Mary R., 7 . .	*Op.*, Oct. 11, 1878. *Dis.* May 17, 1879. *Result*, cured.	Psoas abscess, which had not yet passed into the thigh. Considerable angular curvature of the lower dorsal and upper lumbar vertebræ.
34	Sarah P., 21 .	*Op.*, May 5, 1879. *Dis.*, April 1880. *Result*, cured.	Psoas abscess pointing in the thigh. Had noticed pain over the second and third lumbar vertebræ for twelve months.
35	Catherine C., 15	*Op.*, Feb. 6, 1880. *Dis.*, Sept. ,, *Result*, cured.	Curvature of the spine chiefly in the lumbar region. Psoas abscess. Pain in back.
36	Henry W., 24 .	*Op.*, June 20, 1879. *Dis.*, July 1880. *Result*, in process of cure.	Patient was admitted with disease of the elbow-joint. Had suffered from pain in the back for some time. An abscess was found in the left lumbar region.
37	Mary P., 25. .	*Op.*, Oct. 7, 1880. *Result*, cured.	Psoas abscess. Prominence of first lumbar vertebra. Great pain and tenderness over that part; she could not be touched or moved without crying out. General health very bad.

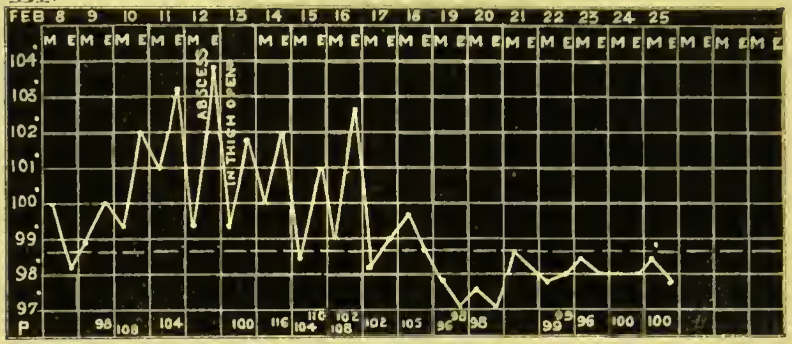
Lumbar Abscesses (*continued*).

Treatment	Remarks
An incision was made into the abscess above the crest of the ilium, and six pints of clear fluid were evacuated. The fluid in the buttock was evacuated by the same incision. Wound stitched up.	The fluid reaccumulated in the buttock, and in the beginning of August, 17 oz. of serous fluid were removed by the aspirator. There was again reaccumulation, and 18 oz. were withdrawn on August 22. On September 4, the old cicatrix was opened and a drainage-tube inserted. The tube got blocked, and there were reaccumulation and high temperature. (The patient was also menstruating at the time.) As soon as free exit was provided for the fluid, the temperature fell. The case then followed a typical course and the wound healed during April 1879. She went home quite well and strong in the beginning of June. When the fluid was let out on September 4, it had a peculiar odour and contained numerous bacilli. These, however, disappeared in a few days, being apparently either dead already or incapable of developing in the serous discharge from the wound. (See T. Chart LXII.)
Opened like No. 17. Drainage-tube inserted, 14 oz. of pus with pieces of bone escaped.	Aseptic course. Wound healed on March 4, 1879. Patient was in excellent health throughout the whole treatment. (See T. Chart LXIII.)
Opened above the crest of the ilium about its middle; 50 oz. of thin pus escaped. Drainage-tube inserted.	Aseptic course. Healed completely on February 25, 1880. Patient improved in general condition from the day of the operation. (See T. Chart LXIV.)
Opened like No. 17, 8 oz. of thick mortar-like pus escaped containing pieces of bone. Drainage-tube inserted.	For some days this mortar-like stuff containing fragments of bone could be pressed out. By February 24, the discharge was serous and the case thenceforward followed a typical aseptic course. The elevation of temperature on February 9, and the following days coincided with the patient's first menstruation. Healed during July 1880. (See T. Chart LXV.)
Opened. Drainage-tube inserted.	Aseptic course. In the summer of 1880, as there was no discharge, even though the sinus had not healed, the patient was allowed to walk about. When the hospital was closed in July 1880, the sinus was still unhealed. He was sent home to be treated aseptically by his own doctor. (See T. Chart LXVI.)
Abscess opened and 24 oz. of pus containing a piece of bone evacuated. Drainage-tube inserted.	Aseptic course. The pain in the back disappeared in a few days and the patient's health rapidly improved. Found healed on June 16, 1881. Patient then well and strong and quite free from pain or tenderness. (See T. Chart LXVII.)

LIII



LIV



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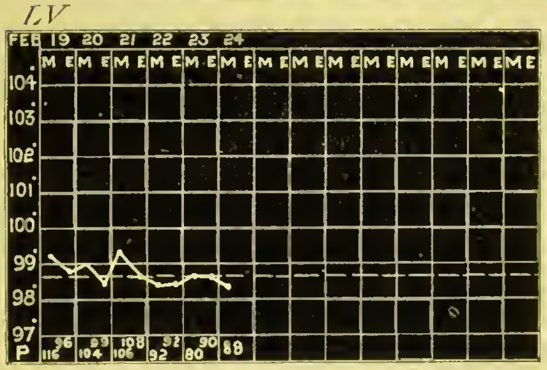

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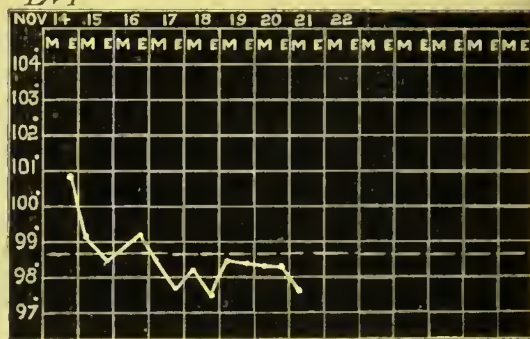

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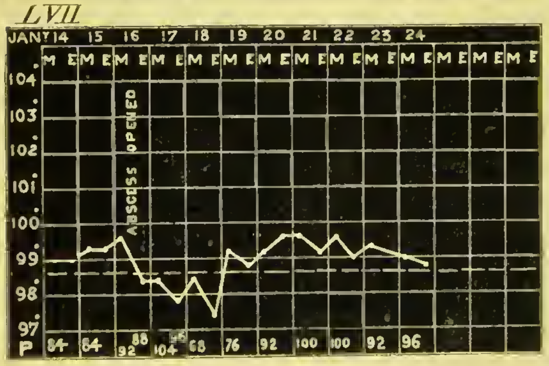

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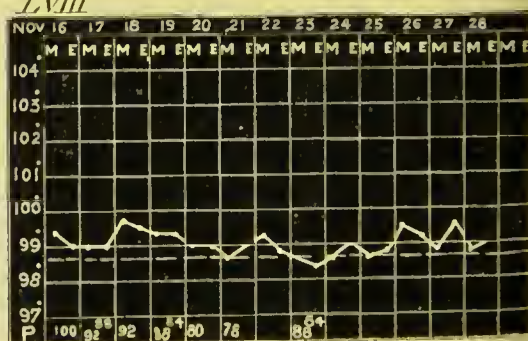

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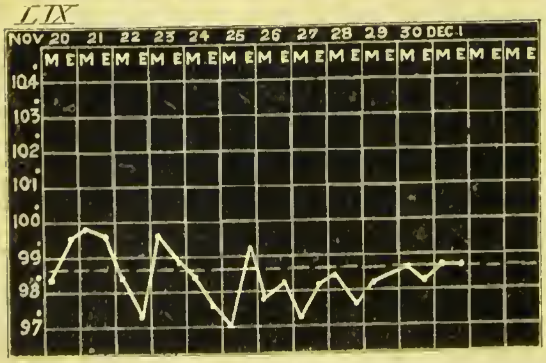
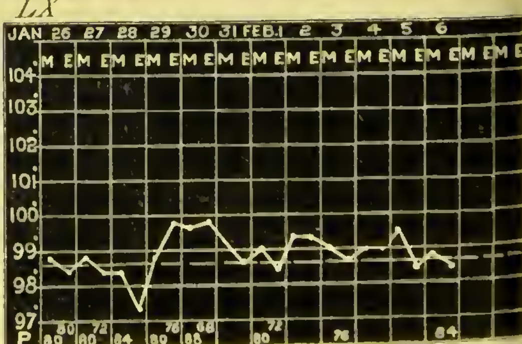
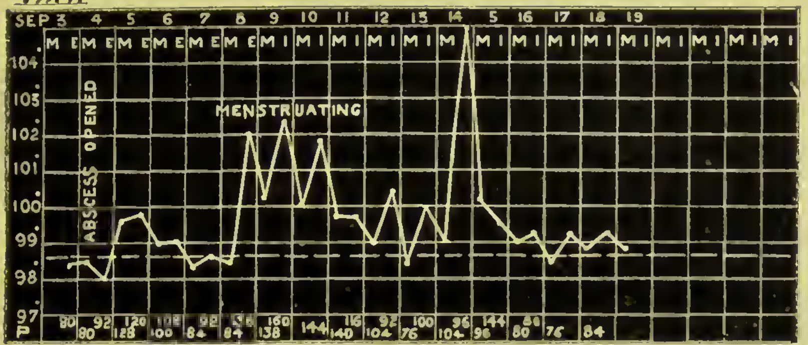


LX

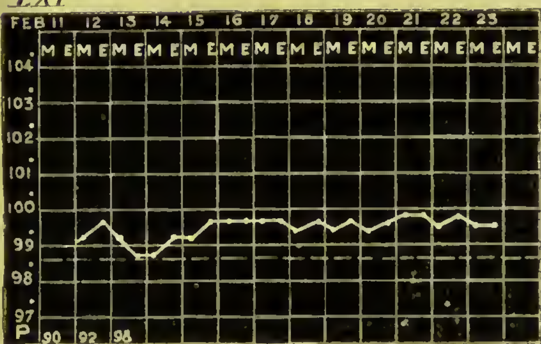


TEMPERATURE CHARTS OF CASES OF PSOAS AND LUMBAR ABSCESS.

LXII



LXI



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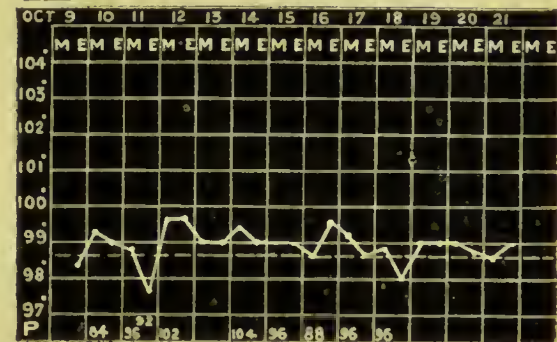

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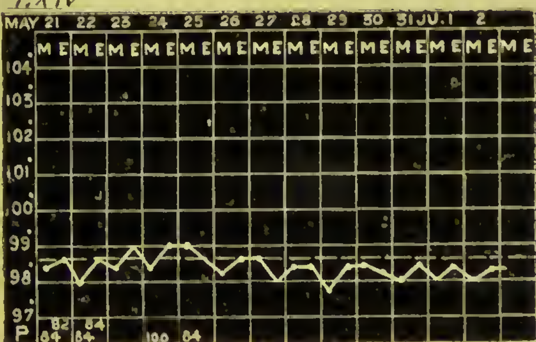
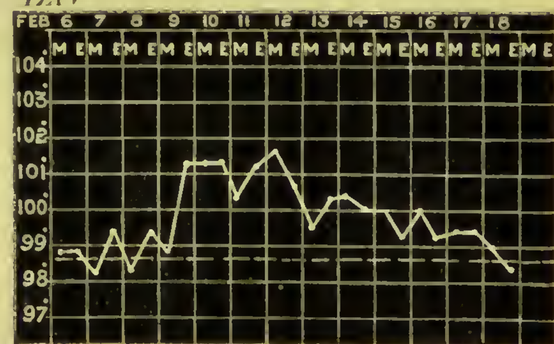
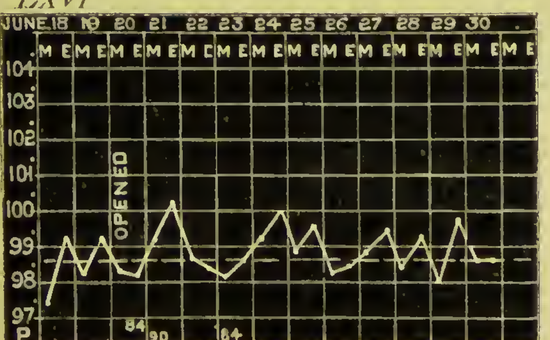


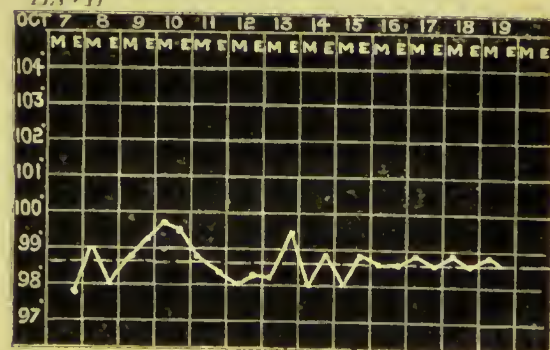
LXV



LXVI



LXVII



TEMPERATURE CHARTS OF CASES OF PSOAS AND LUMBAR ABSCESS (*continued*).

Another case was admitted during these years, a little child with psoas abscess. This was opened aseptically, but the parents insisted on removing the child 3 days after the operation, for no apparent reason. As of course such a case could not be of any value in regard to the effects of treatment, I have not included it in the list.

We have thus 37 cases of abscess connected with disease of the spine treated by free incision and the insertion of a drainage-tube under strict aseptic precautions. Of these 23, or 62·1 p. c., were certainly cured. This figure does not however represent all the cases which recovered, for several of the patients left hospital continuing the aseptic method, and I see no reason why these did not also recover. I would thus consider that 4 other patients (Nos. 3, 7, 18, and 36) were probably also cured, giving a total of 27 cures, or 72·9 per cent. of cures.

Of these 37 cases 4 died in hospital, and 1 is known to have died soon after leaving hospital, giving 5 known deaths in 37 cases, or a mortality of 13·5 p. c. But it may be said: 'The cases which putrefied and were discharged also died;' and no doubt some of them did. Reckoning these cases therefore as also cases in which a fatal result ultimately occurred, we should have 9 deaths in 37 cases, or a mortality of 24·3 per cent. We know however that this percentage is too high, for the boy No. 28 was improving in general condition when he was last heard of, and he very probably recovered, and the result in No. 16 was by no means certainly fatal.

If we enquire into the causes of death we find, that some of them must necessarily be present in a certain number of these patients. Thus Nos. 12 and 24 died of phthisis, while No. 21 died of exhaustion, and on post-mortem examination there was found most extensive disease of the spinal column, so extensive and of such a nature that the chance of recovery under any circumstances was exceedingly minute, if indeed it can be said to have existed at all. No 5 died of a cause quite independent of the lumbar abscess; indeed this case ought to be reckoned as one which was cured of the lumbar abscess, but which died from another cause before leaving hospital. The case shows very well the dangers of a septic as compared with those of an aseptic wound; for for months the patient had had an open

wound treated aseptically without any bad result, but he did not have a septic wound many days before it was attacked by erysipelas, of which the patient died.

The cases in which putrefaction occurred show very well the contrast between the course of cases where the aseptic method has failed (in other words the course of septic cases), and of those where the aseptic method has been successfully carried out. If we look at the causes of failure we shall see that in one or two cases there was good reason for it. Thus in No. 1 the abscess was opened at the upper and inner part of the thigh quite close to sources of putrefaction. It was this case which showed the danger of incisions in this situation, and led to the much better plan of opening psoas abscesses in all cases above Poupart's ligament. The immediate cause of putrefaction in this instance was that the menstrual discharge soaked the dressing and conveyed putrefaction to the wound before the accident was detected. Here also it must be noted, that the application of an elastic bandage along the margin of the dressings had not yet been introduced, and hence the edge of the dressing was not always in apposition with the skin. That putrefaction occurred under these circumstances is not a matter for surprise. No case will go wrong from this cause in future. Then in No. 28 the dressings slipped owing to the extreme deformity of the patient and the difficulty in retaining them in position, but this will probably also be avoidable in the future by careful management. In No. 31 we had the complication of carbolic poisoning—a complication happily extremely rare—and by the use of one or other of the powerful antiseptics now at our disposal we may, I think, reckon that if a similar case should occur again, there will be no necessity for such treatment as would involve the risk of putrefaction of the abscess. Then we have two cases in which no explanation is given (Nos. 16 and 20). These were likely due to the carelessness of the dressers; one (No. 20) occurred during the Christmas holidays, when most of the dressers were away, and when those who were left had generally more work than they could do without hurrying over it. Altogether, I believe that in future the chances of a case going wrong are very small, as indeed we already see from the more regular progress of the last cases. For, if we

look at the results since 1875, we find that 73·3 per cent. of the cases were certainly, and 80 per cent. probably, entirely cured, as against 59 per cent. of certain, and 72·7 per cent. of probable, cures in the preceding four years. But this does not yet give a true idea of the present probabilities, for of the two cases which proved fatal since 1875 one died of an unavoidable cause—phthisis—while in one putrefaction occurred under circumstances which would not now happen, viz. the absence of a fairly trustworthy substitute for carbolic acid. The results of the last few years, both in hospital and private practice, shew that in a patient not affected with phthisis or other dangerous malady a cure is in the highest degree probable.[1]

If we look at the cases which recovered, we see that what I stated at p. 519 is true—that the effect of opening a large abscess aseptically and keeping it freely open by means of a drainage-tube is not a severe attack of fever, nor profuse suppuration, nor hectic fever, but is the relief of the patient from any hectic symptoms from which he was previously suffering, and his rapid return to a normal state of health without any suppuration from the abscess cavity. The change wrought in these patients is very remarkable. Some have been brought into hospital in a very feeble and emaciated condition, apparently rapidly dying, and yet in a few days after the abscess was opened they recovered their appetite, they rapidly put on flesh, and soon gained an appearance of robust health. The cases which were cured were not cured after a weakening confinement to bed, but, when they left their beds, they were in

[1] With reference to all the cases mentioned, I need hardly point out that the results obtained during a number of years in which a method is being developed do not give the *present probabilities* of success. The asep'ic method is much more perfect at the present time than in 1872, and therefore the failures in the earlier period reduce the average results. It is only by failures that advance is made, and the failures in the earlier period have led to improvements which avoid these errors in future. For a very simple example of this look at Case 1 of the psoas abscesses, where the failure led to the selection of a better position for making the incision. In the same way other failures led to the wetting of the deepest layer of gauze, to the use of an elastic bandage, &c., and it is only the results since these improvements which shew the present state of matters and which now give those who employ this method great confidence in it.

good health and strong, while when they were admitted they were in some cases miserable, weak, emaciated creatures.

This success after opening spinal abscesses aseptically has been obtained by several surgeons who have used the method carefully, while, on the other hand, surgeons who have been apparently successful with other wounds have failed in these. This is the case in which perhaps of all others faulty manipulation becomes evident. For in an ordinary wound, as we have seen, the healthy tissues may destroy causes of fermentation should they accidentally enter the wound, but in an abscess cavity like this, such destruction will not occur, and therefore a slight error which might not matter and might escape notice in the case of a wound, will entirely upset the result here. These cases are really tests as to whether a surgeon is thoroughly versed in the details of the aseptic method; and till a surgeon is able to reckon on success in these instances, he ought not to venture on operations of convenience, such as many operations on joints, &c., in which failure is apt to be followed by grave consequences, nor ought he to bring forward his own experience as telling for or against the aseptic system.

When I come to look for comparative statistics on this subject I find none. During the Edinburgh period I do not find mention by Mr. Spence of a single case of this kind, nor do I find reference to them by other statisticians. I have, however, in the history of this subject referred at length to the views of surgeons on the dangers of opening these abscesses, and we have seen that when they were freely opened and kept open, death was looked for in the vast majority of cases. The only methods which yielded any sort of satisfactory results were the valvular method and the method by aspiration.

At the meeting at St. Thomas's Hospital, to which reference has been already made, Sir James Paget said, 'A few years ago I believed that I had never seen a patient recover after the opening of a lumbar or a psoas abscess with a free incision; I could not remember one who had not died before the opened abscess had healed. Of late years I have known such abscesses opened with complete impunity under antiseptic treatment; and there has seemed nothing but this treatment to account for the difference of results.'

CHAPTER XXI.

RESULTS OF ANTISEPTIC SURGERY—(*concluded*).

General consideration of the results. Results of the various methods in saving life. Results in avoiding infective disease. Cleanliness: definition of the term: Mr. Savory's definition and method: cleanliness has not abolished infective disease even in healthy hospitals: cleanliness is a complex method: infective diseases may appear even in the best hygienic conditions. The source of infective disease. Conclusions as to the value of the various methods in preventing infective disease. Deaths from prolonged suppuration after chronic abscesses, compound fractures, &c. Operations on weak or diseased individuals are rendered possible by the aseptic method. Operations otherwise unjustifiable, but nevertheless necessary for the recovery of the patient, may be safely done by the aseptic method. The patient may be made a more useful member of society: joint cases: tenotomy, &c.: compound fracture: dangers of operations of convenience. Local results of wounds treated aseptically: absence of pain, inflammation, &c.: experiments of Yeo and Ferrier: organisation of blood-clot, catgut, sloughs, &c. Histological details of the process: Tillmann's experiments. Temperature in aseptic cases: contrast with septic cases. Local and constitutional course of cases not treated aseptically. Objections to aseptic surgery: carbolic acid poisoning: the surgeon is said to neglect the constitutional state of the patient: expense: trouble: necessity for the spray. Conclusion: great principle of wound treatment is Rest.

We are now in a position to consider the points referred to at pages 365 and 366, and first as to the results of the various methods in saving life.

In Chapter XVII. I have brought forward a mass of evidence to show *what are the results of the various methods in avoiding infective disease*, and I must now refer to this evidence very shortly. We have seen that the aseptic method, when efficiently carried out, has practically abolished infective diseases, and that this result has been obtained whether the hospital was one in which these diseases were only present in small amount, as in Edinburgh, or whether it was one in which, from some cause or other, they were rife; where the hospital

was, as it is said, infected. We have also seen that none of the other forms of antiseptic surgery give the same *certainty* as regards the result, and that the absence of infective diseases increases in direct proportion to the increase in asepticity of the wounds. I have merely to recall the facts from Mr. Lister's own practice, which show the difference in the results of aseptic treatment and treatment by antiseptics carried on by the same surgeon, in the same wards, during the same time, in cases more or less severe. (I cannot say 'of equal severity,' because the cases treated aseptically were, taken as a whole, much more severe than those treated with antiseptics.) The proportion of infective diseases was four times as great in cases treated with antiseptics as in those treated aseptically. But it must further be noted, that the two cases in which the aseptic method was employed and which died of infective diseases, were both cases in which, through error in the manipulations, an aseptic result was not attained [1]; they were, in fact, septic cases. And so it may be truly stated that in *no case in which the aseptic method was efficiently carried out, i.e. where fermentation and micro-organisms were absent from the wound, did the patients suffer from blood-poisoning. In other words, the aseptic method, when efficiently carried out, was, in Mr. Lister's practice, effectual in entirely preventing infective diseases.*

Similar evidence is furnished by Volkmann, Nussbaum, and others to the effect that in the very few cases in which infective diseases occurred, faulty manipulation could be shown, and this is further proved by the facts that these cases occurred at the commencement of the trial of aseptic treatment, while as yet the surgeons were learning the method, and that since they have become thoroughly versed in its use these diseases have disappeared.[2]

[1] That two cases in which the aseptic method was applied died of infective disease does not prove that the keeping of a wound aseptic is not sufficient to prevent those diseases, because in neither of these cases was the wound kept aseptic, a failure due entirely, as has been previously abundantly shown, to faulty manipulation.

[2] It appears to be generally the opinion of those who have had much experience in aseptic treatment, that while pyæmia and septicæmia are readily enough got rid of, greater care is required to prevent the occurrence of erysipelas. The following is the explanation which I would give of this fact.

The other methods of antiseptic surgery are powerful in this respect chiefly in so far as they prevent or interfere with the occurrence of fermentation in the discharges of wounds. And hence it is that among the best means, not entirely aseptic, for accomplishing the purpose aimed at, are treatment by irrigation and the water-bath, or the very free use of suitable antiseptics. The open method and free drainage are also useful, though to a less extent.

It has been asserted by some writers, more especially by Mr. Savory, that the good results following the use of aseptic methods were due to cleanliness, and that equally good results are obtainable by cleanliness, combined with good ventilation, &c., as are got by the Listerian method. By the term cleanliness, as ordinarily employed, is meant the cleansing of instruments and sponges, in some cases the use of new sponges at each operation and of fresh instruments in the dressing of a case, plenty of water to wash the wound with, the use of fresh and clean dressings, and great care in the ventilation of the wards. Combined with these is careful nursing, good surgery, good hygienic conditions, &c. Mr. Savory, however, defines it as 'the prevention or removal or destruction of all matter which may prove poisonous,' and this definition corresponds to some extent with the meaning of the term 'antiseptic surgery' as employed in this work, though I have limited it to the methods which *interfere with the production* of these poisonous substances, rather than to those which *neutralise their effect* after they are formed; and I have also defined more clearly

There seems to be little room for doubt that erysipelas is a disease due to the growth of micrococci in the skin and subcutaneous tissue, more especially in the lymph channels. Now, as we have seen, one form of micrococcus enters aseptic wounds comparatively readily, more readily than other forms of organisms. We have no absolute evidence, however, that it is only one form of micrococcus which can get in in the manner described in Chapter XII. and it is by no means improbable that the form which causes erysipelas might enter with considerable facility. Where, therefore, a wound is guarded altogether against organisms, erysipelas is avoided; but where care is sufficiently relaxed to let in micrococci, it is possible that the micrococcus of erysipelas may also enter, though the causes of other infective diseases are excluded. That they cannot enter so easily as the micrococci previously alluded to is proved by the absence of erysipelas in the practice of those who use this method most carefully.

what the nature of the enemy is with which we have to contend. The mode in which Mr. Savory proposes to meet the requirements of his definition is the following: 'Taking a case, say, of amputation through the thigh, or of excision of the breast, I should treat the wound in the way following. Having carefully arrested all hemorrhage, using most probably the carbolised catgut ligature, and having gently removed any particles of blood-clot that may have lodged on the surface, employing only clean water or sponges just rinsed out of it, I should, without any further interference with the surface of the wound, bring the edges together, adapting these as nicely as possible with silver-wire sutures. I should not in any way attempt to close the wound completely, but I should leave spaces between the sutures, perhaps from one to two inches long. Then, over the course of the wound and for some distance on each side of it, I should place a layer of folded lint which had been previously well soaked in olive or almond oil containing one part in about 50 of carbolic acid. Over this again I should place two or more layers of dry lint, either with or without cotton wool; so arranging this as, by gentle and equable pressure, to secure without any violence, as far as practicable, the accurate adaptation of the surfaces of the wound throughout, avoiding thus any considerable cavity in the interior. I should secure all this by strapping or bandage, or both, so adjusting these that they may be hereafter removed with the least disturbance. I should place the patient and the wound in the most comfortable position possible, having especial care to the fact that fluids, as they form, may flow outwards. . . . As a rule I do not disturb this arrangement for forty-eight hours, although very often I change the dressing and inspect the wound after twenty-four. . . . The dressings are removed with the utmost gentleness, and the state of the wound carefully inspected. Especially is attention directed to whether there is any tendency to the lodgment of fluid; whether that which forms can escape freely; whether there is much tension of the edges. . . . If the wound presented no other evidence than that of satisfactory repair, I should dress it as before, and proceed in this fashion, dressing and examining it daily or less frequently, according to circumstances. But if at the first

dressing, or at any time afterward, the discharge became at all profuse, or the surfaces did not remain in contact, or there were much tension or a blush at the edges, I should forthwith substitute a bread and water poultice for the previous dressing, and probably continue to employ this until at least all the deeper portion of the wound had closed. When I dressed the wound, I should wash it probably from the first with tepid water, perhaps containing some permanganate of potash in the form of Condy's fluid, or other potent antiseptic of the least irritating kind. I should accomplish this, washing out, if I thought fit, portions or even the whole of the interior by the use of a syringe, avoiding contact of sponges or other substances with the wound. I aim here at the utmost possible cleanliness, having at the same time due regard to the avoidance of any unnecessary disturbance, that the process of repair be not interrupted. And withal I endeavour, by means I need not indicate, to secure for my patient the most complete rest and the purest air.' Such is the way in which Mr. Savory proposes to carry out the 'prevention or removal or destruction of all matter which may prove poisonous.' I do not intend to discuss whether or to what extent this method will succeed in fulfilling these requirements; I leave that for the reader of the preceding pages to decide. I quote it as showing what the best surgeons mean by the term 'cleanliness,' and it will be seen that Mr. Savory's description of treatment by scrupulous cleanliness does not materially differ from what is ordinarily understood by that term.

I must now proceed to inquire whether this cleanliness[1] is

[1] It is too much the fashion at the present time to assert that cleanliness, as the term is now employed, existed long ago, and to speak of it as an explanation of the results of the aseptic method which has been overlooked by Mr. Lister. On the contrary, cleanliness as at present understood is one of the leading developments of Mr. Lister's writings. The *disinfection* of instruments, sponges, &c., is the leading feature of his system, and was not attended to before he wrote. The avoidance of silk ligatures by the use of catgut ones is due entirely to him, for, though catgut had been tried before, it had failed, and it was not till Mr. Lister discovered how to prepare it that it was of any use. Drainage also, though introduced long ago, and again brought into notice by Chassaignac, was very imperfectly used till Mr. Lister worked with it, showed its importance, and demonstrated the best mode of employing it. Cleanliness in the common acceptation of the term is not a feature of

really as effectual in preventing infective diseases as the methods of treatment founded on the Listerian principle. If we look at the evidence on this point we shall find, that no amount of cleanliness, ventilation, &c., has succeeded in abolishing infective diseases to the same extent as the aseptic method. This is at once evident if we again look at the comparative results in Mr. Lister's own hands of cases treated aseptically, and of those treated with antiseptics, &c., *i.e.* by scrupulous cleanliness (p. 376). It is still more strikingly evident if we contrast the results obtained by Professor Spence (p. 378) with those got by Mr. Lister in the same hospital and during the same time. For there we find that the total mortality in Mr. Spence's practice was very much greater than in Mr. Lister's, whether we take the results of individual operations or the total results in the two cases. It will also be seen from Mr. Savory's statement at Cork,[1] and still better from the Report of St. Bartholomew's Hospital for the following year (p. 414), that infective diseases are far from abolished by the use of the 'most scrupulous cleanliness' apart from truly aseptic means.[2] This is the result in healthy hospitals.

But, supposing that it were the case that these diseases are abolished in healthy hospitals by cleanliness, free ventilation, &c., there is abundant evidence to show that these remedies are but feeble in hospitals which are unhealthy or, as it is termed, infected. Look for instance at Professor Volkmann's results before and after the introduction of the aseptic method (p. 385). Before he employed it he had used cleanliness in its best sense, that is to say, he had irrigated his wounds with water and with antiseptics, treated them with the water-bath, treated them with antiseptics, &c., and yet infective diseases increased to such an extent that he had resolved to close the hospital altogether. Nevertheless, as soon as the aseptic method was introduced these diseases disappeared. Perhaps the most striking piece of evidence on this subject derivable

Mr. Lister's method, for he is glad to allow accumulation of blood and dirt around the margin of the wound, so long as micro-organisms do not enter this dirt, because it protects the wound from the irritation of the antiseptic employed.

[1] *British Medical Journal*, August 9, 1879.

[2] See also the results of amputations in St. George's Hospital, p. 414.

from Volkmann's experience, is that with regard to the use of thymol (p. 404), showing that where the *antiseptic* employed was inefficient the surgeon might take the most scrupulous precautions with regard to cleanliness and yet fail to exclude infective diseases.

Similar evidence was brought forward by Professor Nussbaum (p. 393), and his testimony is the more striking as it is clear from his results in the country hospital that he really had been treating his cases with 'scrupulous cleanliness' before he introduced the aseptic method, and yet his patients were dying in large numbers from infective diseases in the hospital in town. Nevertheless as soon as the Listerian method was introduced these diseases at once ceased. That in his practice 'scrupulous cleanliness' when thoroughly carried out was without effect is still better shown by the fact to which he alludes, viz. that even after years of aseptic work with absence of infective disease, these diseases, of as violent a type as formerly, were apt to attack wounds not treated aseptically. Many other facts bearing on this subject will be found in Chapter XVII., and I need not recapitulate them here.

It follows from what has gone before, that a method which is ineffectual under unfavourable circumstances has only a limited usefulness when they are favourable; for, as soon as from any cause, accidental or otherwise, the circumstances become unfavourable, the method becomes ineffectual. In truth cleanliness alone is no method. To be effectual it must be combined with careful nursing, efficient ventilation, good hygienic conditions, careful medical treatment, good surgery, and so forth. Remove one of these conditions, and in proportion to the importance of the factor displaced does the treatment become ineffectual. What the aseptic method does is to substitute a unity for a complexity, and thus to render the patient independent of any disarrangement of a number of factors. Exclude the causes of fermentation from wounds and, as the evidence I have brought forward abundantly proves, you at the same time exclude the causes, whatever they may be, of infective disease.

With the exception of a few extremely rare cases which may be left out of discussion, the causes of infective disease enter the body through a wound. If therefore these causes be

excluded from the wound, they may be abundantly present in the surrounding air, and the patient may be in a fit condition, or in other words, may be a favourable soil for their reception, but nevertheless he will remain safe. And thus the surgeon has a feeling of *certainty* that in spite of the condition of the surrounding air, or the receptivity of the patient, the latter is safe so long as the means employed to exclude the causes of infective disease prove effectual. On the other hand, aim merely at rendering the causes of infective disease inert by a number of isolated precautions, and the failure of any one of these from causes overlooked or unknown renders the patient liable to be attacked. There is no certainty, no security, in such a method.

That a patient is liable to be attacked by pyæmia and other infective diseases, even under the most favourable circumstances, when he is not treated aseptically, is most beautifully shown by Mr. Holmes' experience at the Wimbledon Hospital, and I do not think that I need apologise for quoting his words in detail.[1] In order to test whether operations would succeed better in the country than in the town, Mr. Holmes operated on two patients at Wimbledon. 'I commenced,' says he, 'with two cases which seemed to me to be very appropriate for the experiment. One was a middle-aged man suffering from chronic disease of the tarsal bones, a perfectly healthy individual who had never, as far as I could find, had any serious disease in his life, and certainly never suffered from erysipelas. The other was a man broken down by all kinds of dissipation, and, no doubt, to a certain extent a bad subject for an amputation, but otherwise there was nothing very remarkable about the case. I sent these two men down to Wimbledon, and performed amputation on both on the same day, a few days after their admission into the hospital. They were treated in separate rooms, neither room having been used before; one was in one of the wards of the hospital, the other in one of the private rooms. They were separate from each other, but attended by the same nurse; otherwise they were in exactly the same conditions as a man would be in private practice. The rooms were entirely free from all possibility of contamination. They were not attended by medical students at all, but by the resident

[1] See the debate on Pyæmia. *Transactions of the Clinical Society*, 1874.

medical officer of the hospital. There were no other cases in connection with them whatever, and all the other cases in the hospital were simply convalescent cases. I never saw two cases more likely to do well. One of them was certainly a case of amputation which anyone would have expected to recover, merely Syme's amputation for chronic disease. Both of these people died, one of pyæmia, the other of erysipelas. The erysipelas did not attack the stump at all, but simply the head. This appeared on the fourth or fifth day, and was followed shortly afterwards by sloughing of the skin of the back to an enormous extent, a piece as large as a soup-plate sloughing a few hours before his death. The patient died on the fifth day after amputation. The other man died on the seventh day from pyæmia.' Argue over these cases as you like, they simply shew that the contention that cleanliness, isolation, and so on, are sufficiently protective against infective disease is incorrect, for by these methods some unknown factor may be overlooked and the patient become infected.

During the debate at the Clinical Society, evidence to the same effect was brought forward by a number of speakers. Thus the president, Mr. Prescott Hewitt, mentioned the occurrence of pyæmia in his private practice in twenty-three cases. Mr. Hutchinson also brought forward evidence to show 'that pyæmia is extremely common amongst the lower animals, and *it occurs amongst them when they are not crowded in the least, but when they are placed under the most perfect hygienic conditions*.' Mr. Charles Hawkins mentioned that an outbreak of phagedæna occurred in St. George's Hospital in the ward at the top of the house, 'in the best ventilated and best placed ward in the hospital.' Sir James Paget stated that pyæmia was as common in private as in good hospital practice. He says, after enumerating several cases:—'I therefore come to the very clear conclusion that there is really nothing, I will not say in any hospital, but nothing in a well-managed hospital, which contributes to the production of pyæmia.' Dr. Barnes, who had been for some time physician to the 'Dreadnought,' referred to a great improvement which had followed the abandonment of sponges, and the substitution of fresh tow for the cleansing of each wound, and the avoidance as far as possible of

the transference of any contaminating material from one patient to another. Mr. Croft, who had been surgeon to the 'Dreadnought,' said with regard to the permanent effect of these improvements:—'I know that after some alterations had been made in the state of the hospital, an improvement took place in the number of cases of erysipelas, pyæmia, and phagedæna, but, after a time, while Mr. Tudor was there—I am sure if he were here he would bear out what I say—both pyæmia, erysipelas, and hospital gangrene were rife.'

I need not quote other facts mentioned there and elsewhere to shew that cleanliness, free ventilation, &c., are not by any means perfect protectors against pyæmia, but are liable to become ineffective from a variety of causes. I do not of course mean to deny or detract from the value of cleanliness and good hygienic conditions.[1] On the contrary, they are most excellent and do much to abolish infective diseases, more especially if they are associated with free drainage, and chiefly, I believe, with the open method and antiseptic irrigation. My argument merely is, in the first place, that however well carried out, they do not even in the most favourable cases protect the patients *entirely* from risk, more especially in operations, such as on the bones, which are particularly liable to be followed by pyæmia; and in the second place, their success is so much at the mercy of numerous disturbing causes, many of them unknown and therefore to a great extent unavoidable, that they cannot form anything like a guarantee against the occurrence of infective disease. No one could say of any individual case so treated: 'I have no fear that you will die of infective disease, unless you are one of those rare individuals who apparently get pyæmia spontaneously: indeed you need not take the danger of infective disease into consideration.' On the other hand, with the aseptic method, whether the operation be conducted in an 'infected' hospital, or performed on parts particularly liable to be followed by blood-poisoning, there is practically, as the evidence

[1] I do not of course mean to deny that good ventilation is useful even where aseptic treatment is carried out. Good ventilation has been shown to be essential for health, and it therefore ought to be carried out as far as possible in every case. I speak here of ventilation, &c., as *substitutes* for aseptic treatment.

brought forward abundantly shews, security against its occurrence.

The explanation of these facts has been furnished by various writers, and as Mr. Savory has, in his speech during the debate on Pyæmia and at the meeting of the British Medical Association at Cork, summarised the points which are essential for understanding the *modus operandi* of antiseptic surgery, and for showing the necessity for truly aseptic treatment, I may quote what he says: 'We may take a decomposing fluid, inject it into the blood, and produce all those effects which are generally recognised as the effects of pyæmia.' The poison 'is formed during the decomposition of animal fluids, animal fluids in connection with the living human body.' 'Decomposition is unquestionably hastened by exposure' to air. 'Most of all, the introduction of other matter in a state of active decomposition increases vastly the rate of decomposition of the original fluid.' This decomposing fluid 'can, by a syringe, be introduced at once into the blood of an animal, and produce the most terrible forms of blood-poisoning.' 'As we have been already told, the not unhealthy pus, normal pus, may be injected into the circulation, and you do not get as a necessary result, by any means, pyæmia. But pus is an animal fluid, which, of all fluids, is most likely to be found in contact with wounds, and, obeying the law of exposed animal fluids, is exceedingly likely to undergo decomposition; and then pus, undergoing decomposition, will produce pyæmia, as any other fluid will produce pyæmia which is in a state of decomposition.' 'Take pus; you do not want to go to decomposing vegetable and other animal fluids; you may do it with them, but take pus, and with it I could make a case of pyæmia or septicæmia according to order, by the length of time which I kept the pus before I injected it; and I know very well, in experimenting on this subject, one may produce all degrees of the disease, and may say that the chances of getting secondary abscesses are in direct ratio to the length of time an animal lives after it has become inoculated with the poison. When the poison is thoroughly septic, when you have that terrible substance which Dr. Burdon-Sanderson has shown us how to get in the peritoneal cavity of an animal, the blood becomes so poisoned and spoilt that it kills outright, and

there is no time for the secondary effects to supervene.' And then how is it that this fluid can be in contact with granulating surfaces, and yet the patient be free from all symptoms? Simply because the granulations protect the patient against the absorption of the 'terrible substance' in contact with them. There are other reasons which I need not mention here, but the most powerful is that which I have just stated. I again quote Mr. Savory: 'The best work done in late years in this direction are those experiments which Billroth and other persons have performed, showing that where granulations are healthy, when they exist in their integrity, they offer a decided obstacle to the passage of the material from without to within; but when these granulations become destroyed, either mechanically or by other means, whereby they are brought into an unhealthy state, these fluids pass with fatal facility through them and so gain entrance into the blood.' I have already demonstrated that the decomposition of organic fluids and tissues was dependent on the introduction of particles into them from the outer world, and that these particles are bacteria or their spores. The latter point is not absolutely essential for the question at issue, indeed Mr. Savory sums up the essential points as follows in his address at Cork. 'I turn away from any farther inquiry as to the nature of these septic particles. . . *It is enough for us that they are septic; that they can produce and promote putrefaction; and further, that fluids so changed by them may provoke those terrible effects which are only too well known as blood-poisoning.*'[1]

I do not pledge myself to all Mr. Savory's conclusions, more especially where he states that he can get pyæmia and septicæmia according to the length of time that the decomposing fluid has been kept or that the animal has lived after inoculation; but the statements taken as a whole reflect fairly well the present state of knowledge on this subject, and are sufficient explanation of the results of the different varieties of antiseptic surgery in regard to infective disease. They shew that no precautions which do not entirely prevent the growth of organisms in wounds can be trusted to remove the patient altogether from the risks of blood-poisoning, for if organisms are not entirely

[1] The italics are mine.

excluded from wounds, and if these wounds are not very carefully drained, fluids may be confined in the deeper parts of the wound and there undergo decomposition. The use of a bread and water poultice greatly facilitates the putrefaction of the discharge; and thus it comes about that a patient so treated is practically only protected from the 'terrible poison' in his wound by a thin and delicate layer of granulations. If these remain whole and healthy all may be well,[1] but if from some injudicious movement this layer is ruptured, or if from some other cause the granulations lose their vigour, then the patient is liable at any moment to the entrance of the poison into his circulation, and to the consequent dangers.

Mr. Savory considers that the term 'antiseptic surgery' is synonymous with the term 'good surgery.' I would ask, if it is good surgery to leave a patient subject to the risks just mentioned, and dependent for his safety on a number of factors, any one of which may fail to act and upset the remainder; or whether it is not better surgery to keep away altogether from the wound the causes of the formation of this 'terrible substance,' and thus make the patient independent of the numberless accidents which might render him liable to the absorption of the poison, if it were already present. Cleanliness, isolation, ventilation, poultices, drainage with gutta-percha tissue, &c., may be very excellent when suitably combined and carried out, but, as I have already pointed out, this is a system so complex and so impracticable as to be incapable of providing a satisfactory safeguard against infective disease; it is not the simple thing which some hold it to be. The true simplicity, I again say, is where there is only one factor to be considered, as in the aseptic method.

I do not intend to enter in this work into the discussion of the germ theory of infective disease at all. To do so thoroughly would require much more space than I have at my disposal; and further, its discussion might tend to obscure the real principle which is at the basis of antiseptic surgery; for, as I have already pointed out, the tendency in the present day is to bring

[1] This does not by any means always follow, for some forms of micro-organisms may be able to penetrate through healthy granulations, and produce infective disease.

prominently forward the germ theory of infective disease, and thus lose sight of the real points at issue. For my own part, I consider that the evidence in favour of the germ theory of infective disease is overwhelming, and I constantly admire the temerity of those who, often through ignorance of the present state of knowledge on this subject, ridicule it and speak of it as far from proven. It is sufficient, however, for our purpose to recognise that when the precautions taken to exclude micro-organisms are successful, the causes of infective disease are also excluded. This is an absolute rule. Look at any of Mr. Lister's cases which went wrong; they only did so after fermentation had occurred in the wound, after the aseptic method had failed to exclude micro-organisms. And so we may draw this rule, that if organisms are absent from a wound, that wound having been properly examined, the patient is practically safe from the occurrence of infective disease. If, however, organisms are present, he may become liable to these affections from causes depending on a variety of accidents which we cannot consider here. This we may accept without in any way adopting the view that bacteria are the causes of infective disease, for all that we need hold is, that the precautions necessary to exclude bacteria are sufficient also to exclude the causes of infective disease.

That the aseptic system, of all the methods of wound treatment, is the most certain and perfect protector against infective disease, is perhaps even more strikingly shown by the results of operations on healthy joints and bones, which are, under other systems, particularly liable to be followed by infective disease. Such operations have been justly looked on as peculiarly dangerous, and as only warranted under exceptional circumstances, while it is also pretty universally conceded that if they can be performed subcutaneously, *i.e.* more or less aseptically, the danger becomes much less. No other system, however, not even the most scrupulous cleanliness and the most perfect ventilation, has ever enabled the surgeon to cut freely into healthy joints or to operate on healthy bones, and to keep up a communication between the external air and the injured bone or joint, without incurring the greatest danger to the patient, and that danger chiefly from infective disease.

And yet the reader has only to turn to Chapters XVIII. and XIX. to see that such operations can be done with safety under aseptic precautions. As I have already discussed these facts in detail, it would be mere repetition to dwell on them here.

We have thus applied crucial tests to the various systems, with the view of ascertaining to what extent they may be depended on as barriers against infective disease, and we have seen that the aseptic method is the one which has done most in this direction, and, indeed, that under certain circumstances it is the only one which can be depended on at all. It follows from this that, as it protects against pyæmia, even in the worst circumstances, it will be equally certain under more favourable conditions; and as we have seen that no other method of treatment is anything like an absolute safeguard against infective diseases in cases liable to be attacked by it, and as these diseases may occur, as we have just shown, even though rarely, under the most favourable conditions, it follows that unless any sufficient reason exists against its use in any particular instance, Listerism [1] ought to be employed, as far as possible, in all cases, and where it is not applicable the most strenuous attempts ought to be directed to the limitation as far as can be of fermentation in the wounds, and to the avoidance of any disturbance such as movement, or anything which tends to make the granulations unhealthy and thus render the patient liable to the absorption of the products of fermentation.

But death after operations and wounds is not only due to infective disease, it may occur as the result of exhaustion, hectic fever, &c. Among these the most important are the deaths which ensue as the result of prolonged suppurations, most strikingly seen after opening chronic abscesses connected

[1] I need hardly repeat what I hope I have already made sufficiently clear, that there is a difference between the terms 'Listerism' and the 'Listerian method.' Listerism is the great *principle* of wound treatment introduced by Mr. Lister, a principle which may be applied in various ways. The best mode of application of this principle is that worked out by Mr. Lister himself and known as the Listerian method. Some surgeons have introduced various modifications of the method, but they still practice Listerism, though not strictly the Listerian method. That the Listerian method is the best mode at present known of carrying out Listerism cannot be doubted.

with carious vertebræ. In this case we have a chronic disease of the bone, which has led to the formation of an abscess in connection with it. The cause—the chronic disease of the bone—which primarily led to the formation of the abscess, was not very active, and thus the abscess was chronic in its commencement, *i.e.* not attended with any marked symptom of inflammation; the causes—the chronic bone disease and the tension exercised by the pus already formed on the walls of the abscess cavity—which induce the continued formation of pus act very slightly, and thus the abscess increases but slowly, and if the inflammation of the bone cease and the pus be not very tensely confined in a sac, it may become a cheesy mass or even be entirely absorbed. As a rule, however, there is sufficient tension on the walls of the sac to lead to continuance of the pus formation, steady though slow increase of the abscess, and maintenance of the bone disease. So long as the skin remains unbroken this abscess increases very slowly. If now the pus be withdrawn by means of an aspirator or by a trocar and canula, and no causes of fermentation be admitted, it may happen, though in truth very rarely, that there is no reaccumulation of pus, the residue which was not removed by the aspirator is absorbed, the cavity closes, and the disease is cured. In some cases repeated aspirations are necessary to secure this result, but in the majority of instances the abscess steadily increases in size and must at last be opened, or bursts externally. And now if no care be taken to exclude the causes of putrefaction, the state of matters becomes very different from that which existed before the skin was broken. As we have already seen, and as the behaviour of the chronic abscess so long as the skin is unbroken abundantly shews, pus has no inherent tendency to undergo fermentation. We also know that the gases of the air cannot induce fermentation, but that particles in the air and deposited on surrounding objects can. If the abscess is opened without sufficient aseptic precautions, of whatever kind they be, these particles must gain admission into the pus in the abscess cavity, and as there can here be no destructive action of the living tissues on these particles, fermentation occurs. And this fermentation may or may not be putrefactive, but whatever it be, its products are always more or less chemi-

cally irritating; and the effect of the application of irritating chemical substances to granulations is to cause them to suppurate. Further, these irritating chemical substances—the products of this fermentation—are not transiently applied but are constantly present day after day in contact with the granulations, for as we have seen, the 'vital ferments' have an indefinite power of multiplication, and thus there is a constantly fresh supply of the irritating products. The result is the profuse suppuration which constantly follows free incisions into these abscesses, and the consequence of this prolonged and free suppuration is hectic fever, exhaustion, waxy infiltration and degeneration of various internal organs, and ultimately, in the great majority of cases, death. On the other hand, prevent the entrance of micro-organisms, as I have shewn can be done by the aseptic method, and the pus remains as unirritating as formerly There is no more reason for great formation of pus after than before the abscess was opened; indeed, the granulations are relieved from the tension of the pus, and are therefore less irritated than before and secrete less. And then in a few days the greater part of such an abscess cavity closes by adhesion of the granulations, and only a sinus is left leading to the seat of disease. But the granulations lining this sinus do not suppurate because they are not irritated, and hence all that happens is a slight transudation of serum, perhaps not a couple of minims in a week, and this continues till the disease is cured and the sinus can close. Thus, during the treatment the patient is not exhausted by profuse discharge, while he is relieved from the presence of the abscess, which by the tension of its contained pus was keeping up the chronic inflammation of the bone and was a source of constitutional irritation. And thus we constantly see that, where such abscesses are opened aseptically and *kept aseptic*, the patient's health at once begins to improve.

That these are not mere theoretical speculations, but that they are fair deductions from the facts, will be evident to any one who will carefully weigh the facts brought forward in the history of antiseptic surgery and in Chapter XX. For there we see that the only method of treatment which could be said to be of any service at all in these cases, excepting the Listerian

method, was Abernethy's mode of puncturing with a trocar and canula, or of making a valvular incision into the abscess. On the other hand, we have the much better results of aseptic treatment, results not obtainable by the so-called simple methods of cleanliness, free ventilation, &c. I need not go into the discussion of these results again; the facts have been sufficiently given in the last chapter.

Though this is the most striking instance in which profuse suppuration leads to loss of life, yet in many other cases great danger is caused by its occurrence. Thus in many injuries of bone, such as compound fractures, very profuse and prolonged suppuration often follows which may endanger the life of a weakly patient, or may even in the case of a healthy patient lead to amyloid infiltration and degeneration of the internal organs. This is chiefly the case where necrosis of portions of the fractured bone occurs; the necrosis may be due to the injury directly, a portion of bone being deprived of vitality or separated from its vascular connections at the time of the accident, but more frequently it is the result of acute inflammation of the bone, acute suppurative osteitis, which supervenes on the injury, and which is due, as is at once evident from comparison with the behaviour of a simple fracture, to the existence of a communication between the injured bone and the external air, and the consequent occurrence of fermentation and formation of irritating products in the wound. But, if the wound is kept aseptic the fracture behaves exactly as if it were a simple one, the danger which results from the external wound being completely avoided. There is therefore no acute inflammation either of the soft parts or of the bone, and hence no necrosis from this cause; the process of repair at once commences. And, just as in the simple fracture, in the way which will be described more minutely presently, portions of bone detached from their vascular supply do not therefore give rise to suppuration, and indeed, by no means require removal, but become encroached on and removed by the surrounding new tissue. Here also this result depends on the asepticity of the wound, however brought about, whether by the aseptic method, or in rare cases by absolute immobilisation or the formation of a crust. If once fermentation occurs

in the wound, suppuration takes place, detached portions of bone are always separated, and frequently necrosis of living bone results. An abundance of facts which prove these views have also been brought forward. The same remarks apply to lacerated wounds, though in these cases the suppuration is not often so great, or at least so long continued, as to endanger life.

Wounds of joints also, though perhaps most frequently fatal in the first instance from sapræmia, septicæmia or pyæmia, yet when these dangers have become slight, are still serious from the prolonged suppuration which follows. We have fully demonstrated that these dangers are all avoidable by taking measures to prevent the occurrence of fermentation in the joints (see Chapter XVIII.)

There are many other cases, which I need not consider, in which the avoidance of profuse and prolonged suppuration is a matter of the greatest importance, sometimes even of life and death. I shall just mention one example more—acute osteomyelitis. In this disease, if the patient escapes the primary dangers from infective diseases (see definition of the term in Chapter XVII.), he is certain to suffer from prolonged and very profuse suppuration, on account of the death of the bone to a greater or less extent. To avoid these risks amputation is frequently resorted to, but recently Mr. Speuce has recommended the excision of the inflamed bone, and has had some good results from this procedure. Neither of these operations are, however, necessary if the case be treated aseptically. It is generally merely requisite to open the abscess freely, with the necessary aseptic precautions, and to attend to free drainage, and as a consequence suppuration ceases at once or is very slight, and sometimes the wound heals up without any separation of dead bone whatever, or if this occurs it is generally merely a superficial exfoliation. Mr. Lister has had several excellent cases treated in this way. The case of osteomyelitis is of course by no means so typical as the other instances which I have mentioned, because, as we have seen, micro-organisms are always present in the pus of the abscess in connection with the bone; but nevertheless its course, if treated aseptically, is often markedly different from that which it follows when treated otherwise, and this, and the fact that some cases do better than

others, may be explained by the other fact previously mentioned (p. 256), that in many acute abscesses the micro-organisms are probably dead when the pus is evacuated.

Then again, if we consider the case of necessary operations on weak or diseased individuals, we shall find that the aseptic method more than any other has diminished their risks, and indeed, with regard to many, alone renders their performance possible. The good health of the patient is one of the many conditions necessary in the sort of treatment which we have just been considering under the head of cleanliness, free ventilation, &c. If the patient be in a weak state, his resisting power to septic influences is less, and the protecting layer of granulations is less potent to resist the entrance into the system of the 'terrible substance' in contact with it. Let the patient be weak, and he can, for a shorter time, endure the exhausting suppuration which may follow the operation, a time too short, it may be, to permit of recovery from it.[1] This is a point of view from which the subject has been regarded by most surgeons who have adopted the aseptic method thoroughly. Thus Mr. Wood, at the meeting of the British Medical Association at Cambridge in 1879,[2] said of the aseptic method, that one great reason for employing the Listerian method was 'that it saved a considerable percentage of weakly constitutions, upon whom it was absolutely necessary to operate, and who would die unless for its precautions.' Among these instances may be mentioned cases of amputation in patients suffering from phthisis. The difference of opinion which exists on this subject is of course well known, and many surgeons refuse to operate where marked

[1] Operations on patients suffering from albuminuria seem to be less dangerous if treated aseptically than a wound is when treated in other ways. This is easily intelligible, for when not treated aseptically there is frequently absorption of products of fermentation from the wound. In a healthy person this may not be of much consequence, the poisonous materials being probably got rid of in great part by the kidneys. Where, however, the kidneys are diseased this material may not be got rid of, but may accumulate in the blood and poison the patient, or may act on the kidneys and cause them to stop working and lead to the patient's death from uræmic poisoning. Both these dangers are avoided if the case is treated aseptically, for in that case this material is not formed in the wound.

[2] *British Medical Journal*, vol. ii. 1879.

phthisis is present. Of course, where a wound is not treated aseptically, and where healing does not occur by first intention, suppuration takes place, and where the patient is weak this reduces him still further, and, if the wound is large, may only hasten his death. In these cases also the irritative fever which occurs is particularly dangerous. Where, however, union by first intention occurs, irritative fever and suppuration are absent, and the removal of the disease is a relief to the patient. As the aseptic system not only favours union by first intention, but also prevents the occurrence of fever and suppuration in cases where union by first intention cannot take place, amputation in cases of phthisis is a much more hopeful procedure than formerly. Several of Mr. Lister's cases have been striking examples of this.

And further, operations can now be performed which may be necessary for the recovery of the patient, but which would have been almost certainly fatal under the former methods of treatment, and which were therefore considered unjustifiable. This of course follows from the facts narrated in the three preceding chapters, for if an operation such as the incision of a healthy joint may be safely performed for the removal of an inconvenience, it must be equally safe when performed for the removal of something which, if left, would imperil the patient's life. I may quote two facts from Professor Volkmann's speech at the recent International Medical Congress.[1] 'For a large enchondroma in the cortal pleura that occupied the left wall of the thorax, Professor Fischer removed a large piece of the chest wall and ribs, so that the heart and lungs were exposed and an opening as large as a child's head was made, and yet the patient was able to be discharged from the hospital after four weeks.' 'In the case of a large echinococcus of the liver, which in front and at the side was covered with thick layers of liver tissue, and which projected into the thoracic cavity, after resection of the seventh rib, I opened the healthy pleural cavity, which was free from adhesions. The thorax was freely opened, the thinned diaphragm cut into, the echinococcus sac opened, the animal bladder extracted *in toto*, and the patient recovered without complication. A similar operation with like results was conducted by Mr. Israel of Berlin.' On this point

[1] See *Lancet*, August 13, 1881.

also Mr. Wood says: [1] 'And another point from which he' (Mr. Wood) 'had been in the habit of considering it' (the aseptic system) 'was this: that it extended the aim and scope and value to the community of surgical skill in cases on which it would be too risky to operate without the extreme precautions which antiseptic surgery affords. There were a great many operations which they were doing at the present time which would have been considered wild, and which were now by many considered unjustifiable, otherwise than with their improved means.'

There are, however, a great many other points which have to be attended to in the treatment of wounds besides the saving of life; we have to look to what Sir James Paget calls the 'well-doing' of the patient. Now as the aseptic method allows the performance of operations which would otherwise be considered unjustifiable, it is evident that it must in many cases enable the surgeon to render the patient a more useful member of society than he would otherwise be. Take, for instance, any interference with the movement of joints from some cause (say the presence of an exostosis) which would involve the opening of a great articulation in its rectification; here the aseptic method permits an operation which would not otherwise be possible. Look at Mr. Lister's list of joint cases, and see whether or not some of the patients have been rendered more useful members of society by the aseptic method.

Take the simple operation of tenotomy. How impossible it was till the subcutaneous method was introduced; but as soon as a method was found by means of which the causes of fermentation were excluded from the wound, these operations became frequent, and they certainly daily increase the usefulness of many patients. And what was impossible in this department in former days by means of free incision, is possible now; and if for any reason one wishes to see the tendon to be divided, it may be exposed aseptically without fear of the result. And further, in cases of ruptured tendons, cases so apt to leave impaired power behind, one may cut down and sew the ends of the tendon together with catgut and in this way get perfect

[1] *Loc. cit.*

union and a complete restoration of power. This has been done in a number of cases with perfect success, and I may refer to Eschenburg's thesis, published in 1877, for the history of these attempts and their results. I may quote two passages from this thesis which shew a striking contrast between the results of septic and aseptic surgery. The first is taken from Ravoth, who says : 'Selbstverständlich wird man die Sehnennaht vermeiden, wenn keine Hautwunde gleichzeitig vorhanden ist.' On the other hand, Küster writes as follows : 'Es ist nun eines der Hauptverdienste der antiseptischen Wundbehandlung dass sie den Kreis unserer operativen Indicationen wesentlich erweitert hat, dass sie dem Chirurgen mit ruhigem Gewissen und ohne die mindeste Besorgniss an eine Anzahl von Operationen zu gehen erlaubt, welche sonst entweder gar nicht, oder nur unter ganz besonders günstigen Bedingungen unternommen werden durften.' And among these operations are those of stitching ruptured tendons.

This is only one example of the numerous applications to which a method can be put which renders operations practically subcutaneous. Look also at all the operations for ununited and badly united fractures, and for deformities of bones, operations to aid the reduction of dislocations, more especially when of old standing, radical cure of hernia, and so on. These instances are so numerous and varied that I cannot detail them here, but, in the practice of the operating surgeon, cases are constantly occurring in which the advantages gained by means of the aseptic method are of the greatest importance.

While the scope of the operating surgeon is so much increased by the aseptic method, the cause of conservative surgery is also greatly advanced. I have at p. 442 shewn that in cases of disease of joints a cure may be obtained in a large proportion of instances, without resorting to any further operative procedure than the free incision of the diseased articulations. By this treatment not only is the limb retained without any shortening, but also there is frequently a considerable amount of movement in the articulation, a result of the greatest importance to the patient. In the treatment of compound fractures, also, there is much less necessity than formerly for amputation, either primary or secondary. This is hardly a

point which can be treated by statistics, but my own experience —and it is the same as that of others who have practised careful aseptic treatment, and a careful perusal of the cases in Chapter XIX. will demonstrate it—is, that the necessity for these mutilations is very much less than when other methods are employed. A limb must be very badly injured indeed before it is necessary to perform primary amputation: it must not be merely that a joint is opened, or that the bone is extensively comminuted or the soft parts badly lacerated: the questions really are, whether the part beyond will retain its vitality, whether if it does so it will be useful, and whether there is a fair probability of purifying the wound. If the last is probable the chance of recovery should be given, and if afterwards it is found that purification is not successful, and if otherwise the injury is one which, without aseptic management, would demand primary amputation, the operation can be performed in two or three days after the injury—as soon, in fact, as it becomes evident that aseptic treatment is impossible.[1] Again, where primary amputation is necessary it need not be performed above the injured parts, but, with the view of getting a longer limb, where that would be an advantage, bruised and even lacerated parts may be included in the stump; for inflammation will not occur in the flaps, and hence the sloughing which would otherwise take place is avoided. I have myself seen limbs amputated in the practice of septic or semi-antiseptic surgeons which I know could have been easily saved by aseptic management; and I have, on the other hand, known the surprise of surgeons when amputation has been performed through the bruised parts in order to get a longer stump or to save an important joint, and their expectation that the injured parts

[1] This statement may seem contrary to the well-known facts as to the dangers of secondary amputations, but here the case is different; for under ordinary circumstances the question of secondary amputation does not arise till it is found that there is danger to the patient if conservative treatment is persisted in. Here, however, I do not advise that one should wait till the patient is in a dangerous state; I merely say that attempts may be made to save limbs which would otherwise be certainly amputated, and that if it is found that fermentation has not been avoided, then the limb ought, in the majority of these severe cases, to be amputated as soon as it is evident that the attempt to purify the wound has failed, without waiting for the appearance of any constitutional symptoms.

would slough 'when inflammation came on.' But fortunately inflammation is not likely to come on if the wound has been properly treated.

I need not discuss this matter further. If one remembers that an incision, supposing it can be made aseptically, provided also that the wound is kept at rest and otherwise properly treated, is not followed by any local reaction nor by constitutional disturbance, one can readily realise how many little operations of convenience may be performed with benefit and increased usefulness to the patient. It may be argued against such operations of convenience that man is fallible, and that a failure of the method might prove a serious matter to the patient. This is to a certain extent true, but the chances of failure in experienced hands in any given case are very small indeed. The performance of such operations by those just learning the method cannot be too much discouraged. It is too much the fashion nowadays to begin aseptic work with some of these daring procedures, such as opening joints; but such operations, however simple they appear, ought not to be lightly undertaken; indeed they ought not to be attempted till one has had extensive experience in the aseptic treatment of wounds, and more especially of chronic abscesses.[1]

Experience is needed in aseptic work just as in every other department of surgery. Why does the student devote so much of his time to learning anatomy and to acquiring manual dexterity in the use of the knife? Simply in order that he may be able to operate with safety to the patient. Take the operation of ligature of an artery or lithotomy or any other great operation in surgery. When performed by an experienced surgeon it seems very simple and easy of performance, but the young operator finds that many difficulties have to be overcome and many little arts acquired before it can be done in the same way as by the experienced surgeon. One would never dream of allowing a man who had never handled a knife

[1] Surgeons are too apt, even without having had any experience of the aseptic method, at once to introduce modifications which generally are illogical and hurtful. Volkmann (*Lancet*, August 13, 1881) makes the following true remark on this point: 'Most of the unfavourable judgments,' says he, 'passed on this method are due to the fact that surgeons who have not yet learnt to experiment *with it* have already made *it the subject* of their experiments.'

before to commence with one of these operations on the living human body. It is just the same with aseptic surgery. For its proper practice are required a scientific training, and more especially, a training in experimental work with its consequent acquirement of patience and dexterity. Let the beginner commence its practice in simple wounds where but little harm will result from his failures, or, better still, let him try some experiments on fluids contained in flasks. Then, just as the operator feels his way to more serious operations, so the surgeon operating aseptically extends the application of his method ; and just as the experienced operator may proceed with confidence to operations which in the hand of an inexperienced man would be very doubtful procedures, so the surgeon practising Listerism may with confidence in its protection perform operations which would otherwise be unjustifiable, such as many of the operations of convenience of which we have been speaking. He has then chiefly to consider whether the advantage to be derived from the operation is worth the inconveniences attending its performance. Of course if one has to deal with a large number of cases one cannot say that *all* of them will do well : in some one or other the experiment may fail. Where there is a large number of cases or a continuous run of successes the surgeon may become less particular than in any special case which he has determined to keep right, and an accident might occur. But we may fairly put the matter in this way : given any *single* case, say of incision into a joint, the surgeon may reckon with certainty that in *that particular instance* no harm will result from the operation. When, therefore, the surgeon undertakes one of these serious operations he should realise the dangers which are run and the means of avoiding them, and should devote his most careful attention to the case in question throughout the duration of the treatment. If this is done it is right to undertake such operations if their performance can increase the usefulness of the patient.

I have said that operations performed aseptically, where the wounds remain aseptic and where they are properly treated otherwise, are not followed by any local or constitutional disturbance, and I must now say a few words on this point. And

first with regard to the local progress of a wound treated aseptically.

Where the wound is treated aseptically by the method formerly described (where the drainage is efficient, where the stitches are not too tight, and where there is no other local cause of irritation, such as movement), no inflammation occurs; there is no swelling nor redness of the edges, as is so frequently the case in wounds treated otherwise. The skin around the wound remains as pale and as lax as it was when stitched up at the time of the operation; there is no evidence of reaction. Thus it comes that such wounds are quite painless, and their edges may be pressed and handled without occasioning the patient any uneasiness. Hence even after the most extensive operations the patient remains free from pain; indeed, the operation being performed under an anæsthetic, and there being no inflammation afterwards, the whole treatment from beginning to end is unattended by any suffering. Patients constantly express their astonishment that the pain which they perhaps most dreaded is absent; and this astonishment is the greater if the patient has been previously operated on and treated by the older methods.

Then, as the result of this absence of inflammation, the scar is linear and soon becomes hardly apparent. One or two surgeons have stated that the scars after wounds treated aseptically were conspicuous, and that therefore the method was unsuitable for wounds on exposed parts. This statement must, however, rest on some mistake—either protective has not been used or some other error has been committed; for my own experience, which is now very extensive, is, that an inconspicuous and linear scar is one of the most striking results of the treatment.

But not only is there absence of inflammation along the cut edges of the skin, it is also absent from the deeper parts of the wound. Thus there is no suppuration even where the deep structures are not absolutely in contact. The discharge from the drainage tube is purely serous and rapidly diminishes in amount so as to render the drain unnecessary in a very short time.

The importance of this absence of inflammation in the

deeper parts of the wound has been well shewn by the advantages gained by the application of the method to experiments on the lower animals. This is perhaps best illustrated by the experiments recently performed by Professor Gerald Yeo on the brains of monkeys.

In Dr. Ferrier's former experiments on the brains of monkeys for the purpose of investigating the function of the various parts of the brain, no attempt was made to keep the animals alive after the operation, because, as the result of experience, encephalitis was looked on as a certain consequence. The attempts which were made failed owing to the occurrence of encephalitis, with the exception, I think, of one animal which lived for some time. It was therefore deemed useless to attempt to preserve the animals, for the after effects of the injury done at the time of the operation were so complicated by the effects of the inflammation which succeeded, that results of any value could only be obtained from observations made at the time of the operation.

Lately, however (1878–80), Gerald Yeo has performed a number of these operations with aseptic precautions, and the results were extremely striking, and yielded very definite and valuable information. Twenty-six operations were done. In the first case great difficulty was found in retaining the dressings in position, and this animal tore off the dressings. It died in a short time of encephalitis, and was the only one out of the twenty-six in which inflammation occurred. Afterwards a very convenient and satisfactory mode of fixing the dressings was devised, and this accident did not happen again. In the other twenty-five cases no inflammation whatever occurred, as was confirmed on post-mortem examination of the animals which died shortly after the operation, or which were killed after a sufficient length of time had elapsed. As no inflammation occurred, the symptoms which were manifest after the operation were due to the lesion caused at the time, and thus valuable and definite facts were elicited. The gain to physiological observation indicated by these experiments cannot be over-estimated.

Of the twenty-six cases operated on seven died, the remainder surviving the operation permanently, without ever

presenting any symptoms except such as were due to the cerebral lesion. Of the seven deaths, one—the first case, in which the animal tore off his dressings—died of encephalitis; two died soon after the operation, one from chloroform and one from shock; one died six days after the operation from cerebral hemorrhage, and here there was no trace of inflammation; three died apparently from the intense cold of winter, one of these surviving the operation for thirteen days without a trace of inflammation. The wounds always healed by first intention, and the dressings were dispensed with in about a week.

As a result of this absence of inflammation in the deeper parts the scar does not become adherent but remains movable. This is of greatest importance after amputations where so much inconvenience is often caused by the adhesion of the scar to the divided end of the bone.

This absence of inflammation is also well seen after opening bursæ or hydroceles. Of late it has become very much the fashion in Germany to treat hydrocele by making an incision into the sac aseptically, stitching the tunica vaginalis to the skin by means of catgut, and inserting a drainage tube. No inflammation follows this procedure; the discharge becomes very slight, the drainage tube is removed in a day or two, the wound heals, and the hydrocele is cured. (The violent inflammation which follows incisions into hydroceles without aseptic precautions is well known.) Similar results are obtained in cases of bursitis, more especially of bursitis in connection with the sheaths of tendons. Incisions may be made into these, the fluid and solid material evacuated, and a drainage tube inserted, without fear of bad result, and with ultimate cure of the disease without any adhesion of tendons or other accidents which commonly follow such attempts.

In cases treated aseptically the wound heals as a rule much more rapidly than when it is treated otherwise; for where the edges can be brought into contact, healing by first intention is the rule, and even where there is a space left, this becomes filled up with blood clot which rapidly undergoes organisation.

The organisation of the blood clot is one of the most marked features in aseptic treatment. That blood clot may become

organised under certain circumstances is no new fact in medicine or surgery. We all know what happens in subcutaneous injuries. Blood clot and lymph are thrown out, they are gradually removed by absorption, or their place is taken to some extent by new tissue. In a simple fracture blood clot and lymph are present between the ends of the fractured bone. The effused material gradually disappears, and its place is taken by new tissue which ultimately ossifies. So after tenotomy; the space between the ends of the divided tendon is filled up with blood clot and lymph, and these become organised, and thus union again occurs. I need not bring forward other examples to show that blood clot in the living body, when protected by the skin, is gradually removed and its place taken by new tissue. In an open wound not treated aseptically the blood clot generally putrefies, and, as a result, breaks down, liquefies, and is washed away with the discharges; the surface of the wound granulates, and thus healing takes place. Where, however, a wound is aseptic, and where it is protected from other sources of irritation, such as the action of the antiseptic employed, the blood clot in it undergoes the same changes as it would under the skin, for the aseptic method renders wounds practically subcutaneous. Thus it does not break down but remains, filling up the wound with a solid brown mass; no granulations are seen, nothing but this brownish solid material. After some days, if this clot be scratched, it will be found to bleed, implying that new vessels have been formed in it, and on lifting up the edge of the clot a broad margin of cicatrix will often be found. If the clot is left undisturbed it frequently happens that after a time a superficial layer of toughish brownish material (old blood clot) may be peeled off and a complete scar found underneath. In some cases organisation occurs in the clot up to the level of the skin, and cicatrisation spreads for some distance under the superficial unorganised layer, and then by-and-by the remaining central portion granulates, and we have a small superficial granulating sore which rapidly heals. The occurrence of granulation may in this case, I think, be due to the entrance of micrococci, which as we have seen sometimes occurs at a late period of the case. If there is much movement of the wound, or if no protective

be employed, this organisation may also be imperfectly or not at all observed. The organisation of blood clot has been demonstrated by several writers, and therefore I need not describe it minutely. The process essentially consists in this: young cells (whether white blood corpuscles or derived from the connective tissue, or both, is not yet determined) pass into the blood clot and gradually form fibrous tissue and vessels, which become connected with already existing ones, according to the various well-known methods of vascular formation. This process gradually extends to the surface till, after some days, as I have said, the blood clot bleeds when scratched. When organisation has sufficiently advanced, the epidermis spreads from the edge. The original blood clot takes no active part whatever in this process: it forms a mould in which the young cells develop, and is gradually either used up as pabulum for these cells, or removed by absorption. Here, just as when subcutaneous, the original blood clot disappears, and its place is filled by young tissue which developed *in* it not *from* it.

Sloughs are served in the same way. When a part under the skin dies, as is frequently the case where infarcts occur, it does not necessarily separate, it is gradually removed by absorption and its place is taken by new tissue. In the same way a slough of the skin if kept aseptic, and not imbued with carbolic acid or otherwise rendered irritating, does not separate but goes through the same process as the blood clot. The ordinary process of separation of sloughs is the occurrence of inflammation at the line of junction of the living with the dead tissue owing to the irritating character of the latter. (The slough causes inflammation because it is undergoing decomposition, for the products of decomposition are irritating.) But if the slough is not chemically irritating it does not cause this inflammation; it does not irritate the living parts around. Then the young cells pass into it as into the blood clot, and the whole process is exactly the same. Thus in lacerated wounds, if rendered aseptic and kept free from the irritation of the antiseptic employed, suppuration and sloughing do not occur: the raw surfaces and the interstices between the torn pieces of tissue become filled up with blood clot, the process of organisation just described goes on, and the whole

or the greater part of such a wound may heal without the occurrence of granulation, suppuration, or sloughing. A similar process has been observed in dead bone kept aseptic and not loose: the granulatious gradually encroach upon it, and it soon entirely disappears by absorption.

The same process occurs in catgut introduced into a wound, whether for the purpose of ligaturing vessels or as a drain. The young cells gradually infiltrate the catgut from without inwards,[1] develop into fibrous tissue, remove the origiual catgut, and take its place. The ordinary process of separation of a silk or hempen ligature is, that fermentation occurs in the discharge which has soaked into it; the ligature thus irritates the walls of the vessel on each side of it; these granulate, and then suppuration takes place at the point of contact of the dead part included in the loop of the ligature with the living tissue. When suppuration has occurred all round on both sides the ligature is loose and may be pulled away. Secondary hemorrhage results when the portion of the vascular wall which has undergone degeneration into granulation tissue as a consequence of this process is not strong enough to resist the blood pressure. If, however, a hempen ligature be applied around a vessel and the wound be kept aseptic, the ligature remains chemically unirritating and may never separate at all. I have seen this well illustrated in a case of amputation where the surgeon operated aseptically, but tied the vessels with hempeu ligatnre and left the ends hanging out: the ligatures would not separate but had to be broken short inside the wound. If now catgut be employed it is a material which is rapidly removed by the young cells which, as I have previously said, infiltrate it, and its place, as a ring round the vessel, is taken by this young tissue, which rapidly becomes organised and replaced by a ring of newly formed fibrous tissue. Thus where a catgut ligature is employed there is no ulceratiou of the

[1] The great advantage of the new catgut prepared with chromic acid is that the cells have a great difficulty in penetrating it, and therefore only erode its surface, and thus a stitch which has been left in the wound for some days, though narrower where it has been in the tissues, is still firm and free from cellular infiltration. With catgut prepared by the older method cells infiltrate it in a few days. With unprepared catgut the cell infiltration and the absorption of the thread is a very rapid process.

coats of the vessel, but after a time a ring of newly formed fibrous tissue will be found in the situation of the original ring of catgut. For these reasons also the risk of secondary hemorrhage is reduced to a minimum.

The facts with regard to the organisation of the catgut ligature were described by Mr. Lister in his first publication on the subject as long ago as 1869,[1] and as some of his statements have been misunderstood I may quote what he says. It has been supposed by some that he held that the dead blood clot and that the dead catgut became revivified in the tissues. Such an idea is of course utterly absurd and was never entertained nor expressed by Mr. Lister. Indeed, he speaks as follows as regards the blood clot, and applies his remarks to catgut: 'Thus the dead, but nutritious mass, had served as a *mould* for the formation of new tissue, the growing elements of which had *replaced* the materials absorbed, so as to constitute a living solid of the same form.' Mr. Lister tied the carotid artery of a calf in two places with a ligature of catgut (in one place with two ligatures), and a month later he killed the calf and examined the parts. He says: 'The two pieces of catgut which had been tied round the vessel at the distal part had become, as it were, fused together into a single fleshy band, inseparably blended with the external coat of the artery. The knots were nowhere discoverable, and the only indication of the end which had been left long at the time of the operation was the presence of a black speck' (the original material contained dark mineral impurities) 'here and there upon a delicate thread of cellular tissue in connection with the vessel. The cardiac ligature was in like manner continuous in structure with the arterial wall. The short ends had disappeared; but the massive knot was represented by a soft smooth lump, which appeared at first entirely homogenous, except that it was speckled with dark particles as before referred to. On section, however, I discovered in the interior of the mass, and lying close to the wall of the artery, a small residual portion of the original knot, of comparatively firm consistence, and with the threefold twisted character of the cord plainly visible. It was quite distinct from the living tissue, so that it could be readily picked

[1] *Lancet*, April 3, 1869.

out from its bed with a pair of needles.' Here almost all the original catgut had been removed, but it had served as a mould for the development of new tissue which had taken its place, and which retained the form of the mould in which it had grown. Mr. Lister describes the microscopical appearances as follows: 'A bit of the residue of the peritoneal thread, having been teazed out with needles in a drop of water, presented, like a fresh piece of peritoneum, the wavy bundles of parallel fibres characteristic of perfectly developed fibrous tissue. Adhering to the surface of the remnant of the ligature was some soft opaque material, readily washed off with water, consisting of corpuscles of different forms, most of them caudate or fibro-plastic, but some spherical, though not resembling those of pus; and here and there fragments of the original peritoneal tissue, affected more or less with interstitial cell-development. At a short distance from the remains of the old thread, the fleshy material which had been formed at its expense proved to be a most beautiful example of fibro-plastic structure, the coarse fibres which mainly constituted it being composed of very large elongated cells, often containing several nuclei, and presenting in their course branchings and thickenings of various forms. Here and there were some fibres more perfectly formed, and also cells of a more rudimentary character. Again, the band which had resulted from the organisation of the two fine threads of catgut, which, from the smallness of their bulk, had no doubt vanished early, having had longer time to perfect its structure, was a comparatively well developed form of fibrous tissue, consisting of coarse fibres rather than of elongated cells, being thus intermediate between the merely fibro-plastic material of more recent growth and the completed texture of the original thread.'

A number of writers have described the changes which blood clot and portions of dead tissue undergo in the process of organisation. One of the most interesting and thorough investigations on this subject has been made by Dr. H. Tillmanns of Leipzig.[1] Tillmanns took portions of the liver, kidney, spleen,

[1] Experimentelle und anatomische Untersuchungen über Wunden der Leber und Niere. Ein Beitrag zur Lehre von der antiseptischen Wundheilung. Virchow's *Archiv*. Bd. 78, 1879.

and lungs of rabbits, and hardened them in absolute alcohol for one to three weeks or longer. Pieces of these hardened dead tissues were then introduced with aseptic precautions into the peritoneal cavity of rabbits (in each case several pieces were used); after some days the animals were killed and the state of matters investigated. Twenty animals were experimented on, and into their peritoneal cavities about 100 portions of tissue were introduced. The animals did not appear the worse for the operation; the temperature remained normal, and they seemed well. Of these twenty animals only two died, both of acute peritonitis: in one case an error was committed in the treatment, the stitches were removed too early, and the intestines protruded: in the other case the animal was suffering before the operation from chronic peritonitis which afterwards became acute. When the animals were killed early, in a day or two after the operation, the masses of tissue were found to be adherent to some part of the peritoneum, and sometimes two pieces of tissue were attached to one another. Where fourteen days or more were allowed to elapse, the portions of tissue were found firmly adherent and much diminished in size, evidently undergoing absorption; in some places there was only a thick layer of new material containing a pulpy mass in its interior. In one animal into whose abdominal cavity a whole kidney had been introduced, and which was allowed to live for forty-seven days, the kidney had entirely disappeared; the only thing noticeable was that at one part of the omentum there was a thickish tough spot, where probably the absorbed kidney had been attached. On investigating the process microscopically the following were briefly the appearances found: After twenty-four hours the mass of tissue is, as I have just said, adherent to the peritoneum and surrounded by a layer of soft new material—lymph. Any defects which existed in the margin of the specimen are filled up with this soft mass. This new material when examined is found to be composed of countless numbers of cells, which Tillmanns holds to be white blood corpuscles. If two pieces of dead tissue lie close to each other, they become adherent to each other by means of this material. If these tissues are examined at a later period, say forty-eight or seventy-two hours after their introduction,

these cells are found to have increased in number and to be no longer confined to the outside of the organ, but to have penetrated into it where possible, forming, as Tillmanns puts it, streets and pathways of cells through the tissue. Thus, for example, in the case of the liver these cells penetrate in the first instance along the streaks of connective tissue which lie between the lobules, entering first those channels which are largest but gradually spreading along the smaller ones. At this time the cells have already begun to develop to higher tissue, and not merely round cells, but also elongated spindle-shaped cells undergoing further development, are found. Fig. 39, Plate V., represents this: to the right is seen the old liver cells, and to the left the new cells which have penetrated along the interlobular connective tissue; at the upper part these cells have already become spindle-shaped. This process gradually goes on, the young cells penetrate more and more among the dead materials, which gradually disappear by absorption, their place being taken by this young tissue which has come from without. This tissue rapidly undergoes further development into fibrous tissue, vessels, &c., according to the well-known processes (see fig. 40, Plate V.). The contraction of this young connective tissue and the further changes which it undergoes lead to the disappearance of the original mass and the formation of a cicatrix at its site, which also, as time goes on, tends to dwindle and disappear.

Thus the replacement of blood clot, sloughs, and other dead tissues, in the living body by new material—their organisation, as it is commonly called—is no longer a fact resting on clinical experience alone, but is a process which has been traced step by step under the microscope. What at first sight seems remarkable, what is certainly something new, is, that this process occurs in an open wound. But when the whole facts are carefully considered, it will be seen that this fact is only one which might have been expected, and that it is quite in accordance with well-known facts in pathology.

While there is this absence of local disturbance in wounds treated aseptically, the constitutional state of the patient remains good; in fact, if he has not lost much blood during the

operation, or if the operation has not caused shock, he is, on recovery from the effects of the chloroform, practically as well as before. His appetite is perfect, and I may say here that after operations performed aseptically there is no reduction of diet even for a few days; an hospital patient remains on full diet, and a private patient may have anything he fancies provided it is wholesome, and the more nutritious the food the better. Indeed, after the opening of a psoas abscess, or after an operation which rids the patient of some depressing disease, such as a carious joint, the appetite which was previously very imperfect returns in a few days, and hunger becomes the

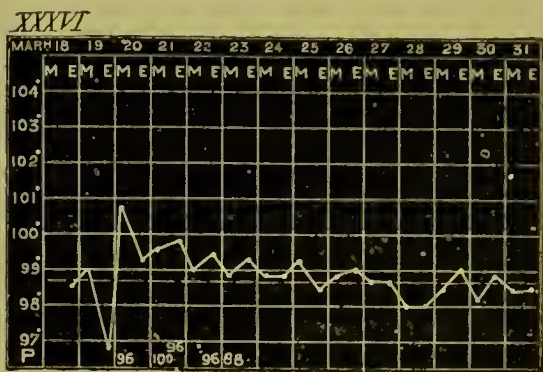


FIG. 80.—TEMPERATURE CHART.

Temperature chart from a case of MacEwen's operation for double genu valgum (Case 69, p. 488).

patient's chief trouble. At the same time there is no fever, as so frequently occurs after operations treated otherwise. I do not propose to discuss here the subject of temperature after operations; to do so would require much space, and our knowledge of the origin and regulation of temperature changes in the living body is as yet so imperfect that we could not come to any definite conclusions. I shall therefore content myself with referring to the following facts.

After an operation performed aseptically, and in which there is no cause of elevation of temperature, such as tension, the temperature remains normal; or if the operation has been at all extensive, the following changes are observed, of which Chart XXXVI., fig. 78 (Case 69, p. 488), is a very good example. In this case both femora were divided, *i.e.* a compound fracture of each femur was made, on the same day: the temperature previous to the operation was 99° F.: on the evening of the

operation it had fallen to 96·8°: on the following morning it was 100·6°, and then it again rapidly fell to the normal, being on the same evening 99·3°. As a rule the temperature after operations performed aseptically resembles this example: the temperature is below the normal on the evening of the operation, it then rises to or above 100°, reaching its highest point on the following morning or evening, and then rapidly falls to the normal line again. In some cases before reaching the normal it fluctuates for a day or two between 99° and 100°, but this fluctuation is not within the range of fever temperature but within the normal range.[1] It is very seldom that the pulse rises in equal proportion, indeed it generally remains normal.

This normal[2] temperature may be disturbed for various reasons, but especially when tension and retention of secretions occur. The elevation of temperature which follows imperfect drainage is often very marked. Among the cases narrated in detail there is no good example of elevation of temperature from tension: I may therefore quote the following instance. A little boy had an unreduced dislocation backwards of the bones of the forearm of six weeks' standing. On December 9, 1876, Mr. Lister cut down on each side of the joint, opened it, and succeeded, after detaching the muscles from the condyles of the humerus, in reducing the dislocation. The parts were very tense after reduction, and the primary rise of temperature reached 101° on the day following the operation, and remained at that level for thirty-six hours; it then fell rapidly as usual. On December 14th Mr. Lister moved the joint for the first time. That evening there was profuse hemorrhage, which ceased on removing the dressing. A fresh dressing was applied, but the hemorrhage went on into the limb, which was next day very much swollen and distended with blood. The temperature rose rapidly and continuously till it reached 104·4°. On December 16th incisions were made into the arm to evacuate the blood

[1] It has been pointed out by Wunderlich that after disturbance of the temperature the curve often fluctuates for a day or two before regaining the normal.

[2] Mere elevation of temperature without other symptoms cannot be called fever. Fever is indicated by a combination of symptoms, and an elevated temperature is only one, though the most striking, feature of the febrile state

clots; on the following day the temperature began to fall, and on the morning of the 18th was 100·8°. After some oscillations about this height it rapidly fell to normal. Here in the first instance, coinciding with tension of the parts after the operation, the primary rise reached 101°, and did not fall at once, but remained for some hours at that height. Then it fell; but the parts became greatly distended with blood, there was great tension, and coincidently with this the temperature rose rapidly and to a considerable height, and again fell when the tension was got rid of. The pulse at the same time increased in rapidity, being on one occasion 144.

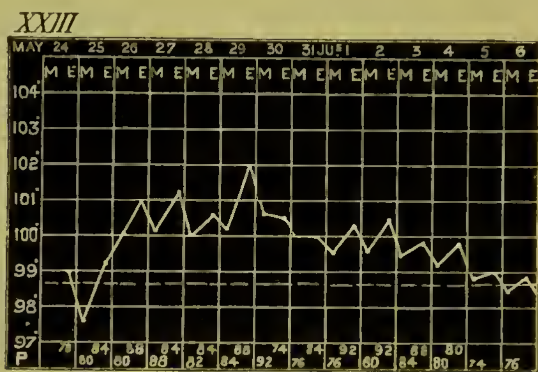


FIG. 81.—TEMPERATURE FROM A CASE OF COMPOUND FRACTURE, IN WHICH THERE WAS GREAT DIFFICULTY IN RETAINING THE FRAGMENTS IN POSITION (CASE 27, P. 472).

Why it is that the accumulation of discharges and the consequent tension should be accompanied by rise of temperature, often very rapid and high, is a very debateable question, and one which can hardly as yet receive an answer. At first the impression was, that this rise of temperature was due simply to the effects of the tension on the nervous system; that it was, in fact, a nervous phenomenon. There can be no doubt that several instances of elevation of temperature in children and hysterical women after operation are in some way or other reflex phenomena. Thus in a child the presence of a tight stitch may be accompanied by a rise of temperature, which subsides as soon as the offending cause is removed. Probably this is the reason for the rise of temperature in Case 27, p. 472 (see fig. 79). In this case great difficulty was experienced in keeping the upper fragment in position and there was therefore

constant disturbance of the parts. At the same time, it must be observed that the pulse rate had not increased in rapidity in proportion, and the patient did not feel out of sorts; with the exception of the elevated temperature there was no other symptom of fever. Here there was no fermentation in the wound and no retention of discharges, and therefore it seems probable that it was a nervous phenomenon. On the other hand, there are cases in which there may be great tension, as after subcutaneous bruises, or local disturbance of the parts as in some forms of joint disease, without an elevation of temperature at all corresponding to that which occurs when the discharges of wounds are retained. And also, the weight of evidence derived from experiments on animals seems to go against the view, that mere disturbance of the nerves of a part is a frequent or probable cause of the marked elevation of temperature which sometimes occurs in cases where there is retention of discharge. Indeed, according to Wunderlich,[1] the application of external irritants has the effect of lowering the general temperature rather than of raising it, and the same author states that Heidenhain has found that irritation of sensory nerves constantly and suddenly lowers the temperature, except after division of the medulla oblongata from the spinal cord or when fever is present. On the other hand, it has been demonstrated that the products of inflammation (the serum of pus, &c.), when introduced into a healthy animal, cause a rapid rise of temperature, which soon passes off if no further injections are made.[2] It is thus quite possible, that when discharges are retained by the blocking of a drainage tube or for any other reason, the fever which follows is due, at least in part, to absorption into the circulation of the retained materials. (In most cases of elevation of temperature from tension the pulse is

[1] *On Temperature in Disease.*

[2] That the products of inflammation should cause fever is not surprising. For, in inflammation, not only do liquor sanguinus and corpuscles pass out of the blood-vessels, but the tissue cells, which are constantly taking material from the blood and altering its constitution—in fact acting as ferments—are, I think there can be no doubt, stimulated to increased activity; their function is also probably perverted, and the changes which they produce in the nutritive materials with which they are supplied may be different from those which occur in health, and may lead to the formation of substances which, when absorbed into the circulation, cause fever.

correspondingly quick, and the patient feels ill—in fact, he has fever. On the contrary, where it is a merely nervous phenomenon, I think, though perhaps I may be hasty in this conclusion, that the pulse does not increase in rapidity to a corresponding degree.) This is, to my mind, the most probable explanation of the high temperature which occurs after ovariotomy in Mr. Thornton's practice.[1] Mr. Thornton does not drain the peritoneal cavity, but stitches it closely up. The consequence is, that the fluid effused from the divided pedicle or from other injured parts is absorbed by the peritoneum, and although Mr. Thornton's wounds are aseptic, yet he has fever in many cases. This fever, however, is not fatal, for as the pedicle heals the discharge diminishes and the temperature falls. Here tension cannot be at work; and further, surgeons who drain the peritoneal cavity, and who at the same time treat their cases thoroughly aseptically, do not meet with this high temperature. Which of these two is the cause of the elevated temperature in tension, or whether both may not play a part, are questions which cannot as yet be definitively settled.

Though tension is the most important cause of elevation of temperature in aseptic cases, there are other minor causes, such as retention of fæces, the occurrence of menstruation, &c.

What the meaning of the transitory rise of temperature after aseptic operations is, it would be difficult to say. In some cases no doubt, where much blood has been lost, it is merely the rise which normally occurs in these circumstances. It seems to be established in the case of the lower animals, that after blood-letting, though the temperature may fall in the first instance, it generally rises to a considerably higher level than that at which it stood before the blood was taken. Similar facts have been observed after blood-letting in man. This rise of temperature after loss of blood is probably the explanation of the curve in Case 16, p. 430 (see fig. 80). Here the ankle joint of a hæmophilious child was opened and hemorrhage occurred from the cut surface on various occasions during the following three days; there was no fermentation of the discharge and no tension. Nevertheless loss of blood cannot always be the cause, for what we may call the 'aseptic curve' occurs in

[1] *Medico-Chirurgical Transactions*, 1881.

cases where little or no blood is lost. In aseptic cases it is probably a nervous phenomenon, more especially as the pulse rate in no way corresponds. I cannot discuss this matter further, as much space would be required, and we do not yet know enough about the origin and regulation of the temperature of the body. One fact is, however, apparent, that besides the ordinarily recognised elevations of temperature after operations *there is a transitory elevation which occurs soon after the operation and as an immediate result of it*, and which can be readily recognised when all other disturbing causes are excluded. I have not met with any instance of the high temperature which Volkmann has after a large proportion of his operations, and which he has termed 'aseptic fever,' and I do not understand it at all.

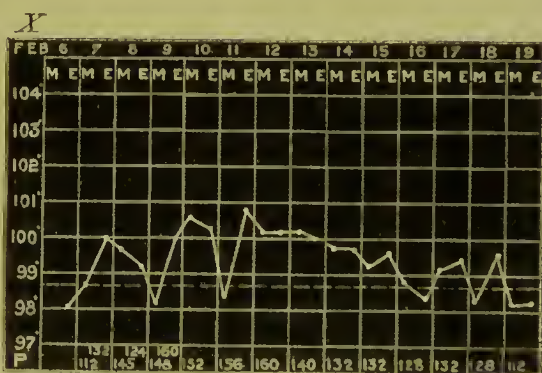


FIG. 82.—TEMPERATURE CHART FROM A CASE WHERE THE ANKLE JOINT WAS INCISED IN A HÆMOPHILIOUS PATIENT, AND WHERE HEMORRHAGE RECURRED SEVERAL TIMES (CASE 16, P. 430).

If one compares the temperature in cases which are treated aseptically with that of those which are not, the contrast will be found to be very marked. I do not of course by any means intend to say, that the temperature is always high after operations which are not treated aseptically; far from it. Many wounds not treated aseptically heal by first intention, and in these there is, of course, no elevation of temperature or merely the 'aseptic curve.' Further, in many wounds in which fermentation of the discharges occurs the discharge is drained off and but little can be absorbed; and in other cases the wounds are small, or the conditions for absorption are not favourable. In these instances there will often be no marked elevation of temperature. But in a great number of severe

operations treated by the ordinary methods of cleanliness, as described at p. 542, there is marked elevation of temperature—traumatic fever—and in some cases this passes into a septicæmie or pyæmic temperature. Look at the temperature chart of Case 22, p. 434 (see fig. 81), and contrast it with that of Case

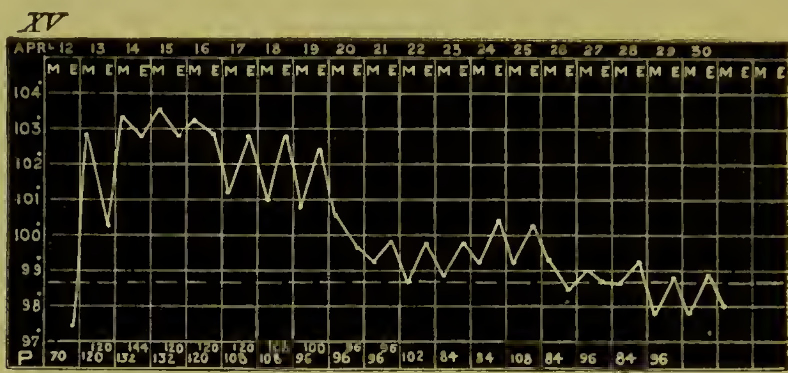


FIG. 83.—TEMPERATURE CHART FROM THE CASE OF REMOVAL OF LOOSE CARTILAGE FROM THE KNEE-JOINT IN WHICH FERMENTATION OCCURRED (SEE CASE 22, P. 434).

15, p. 430 (see fig. 82). In the former case we have a small operation performed on a joint, but fermentation occurred in that joint. As a result we have a severe attack of fever. (Here it is interesting to note that there was no *putrefactive*

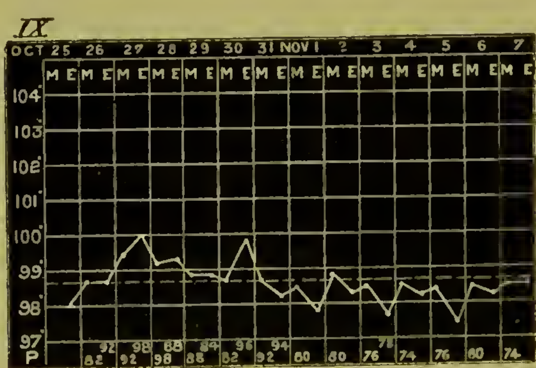


FIG. 84.—TEMPERATURE CHART FROM A CASE OF OPERATION FOR RECENT FRACTURE OF THE PATELLA (CASE 15, P. 430).

fermentation.) In the latter case we have an operation of greater severity, but the causes of fermentation were excluded, and there is a correspondingly normal temperature. The difference between aseptic and septic temperatures is also often very marked after serious injuries or operations such as com-

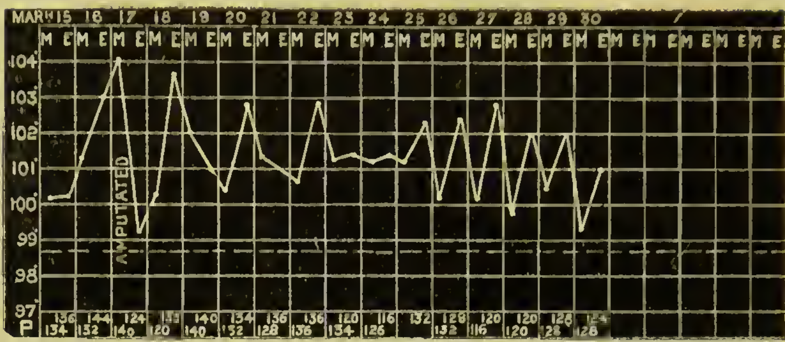
pound fractures. Contrast the temperatures in the cases of compound fracture. In eight cases of compound fracture produced accidentally putrefaction occurred. In four of these (Cases 14, 17, 20, 29) it is stated that the temperature ranged for several days after the injury between 100° and 103° or even higher. In one case (No. 4), though there is no statement as to the temperature, there can be no doubt, from the description of the case, that it was high. The temperature charts of three cases are given here, and in two of them (Cases 12, 26) it will be seen that traumatic fever was present, while in only one (No. 40) was there a normal temperature. In one case (No. 58), in which putrefaction occurred after operation, amputation was performed chiefly because the temperature was rising rapidly; and in another, of which the chart is given (Case 68, Chart 35), there is little doubt but that some form of organism got in, and here also we have a high temperature. Contrast these with the highest temperatures in cases of compound fracture produced by the surgeon and treated aseptically. The difference is so marked that I need not dwell on it.[1]

If we contrast the local and constitutional course of wounds which are not kept aseptic with the foregoing description of aseptic wounds, we see a very marked difference.

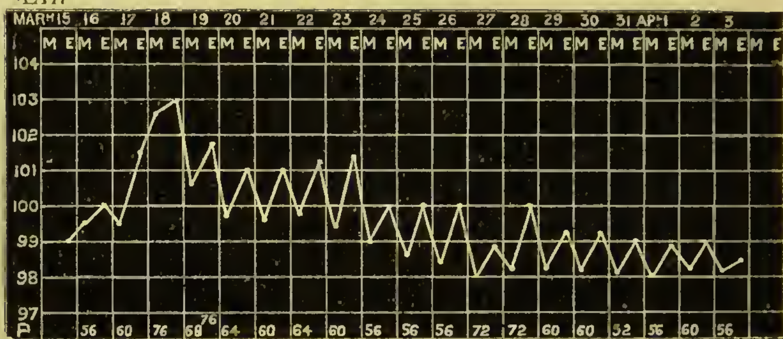
Look at the cases treated with antiseptics but not aseptically in the foregoing tables. In one case of wound of joint (No. 3) putrefaction was not avoided, and the case therefore became, as I have previously pointed out, one treated with antiseptics but not aseptically. Here fever and inflammation set in, and threatened to be so serious in their results that it was considered advisable to amputate. In one case of operation on a healthy joint (No. 22) the wound was not kept aseptic, and thus the case became one treated with antiseptics but not aseptically. (It was dressed throughout with the ordinary gauze dressing,

[1] With regard to the temperature charts published in this work, I wish to state that they have not been in any way selected; I publish all the temperature charts which I have been able to obtain. Till 1877 temperature charts were not in use in Mr. Lister's wards; the temperatures were noted on a card, and it was seldom that the clerk took the trouble to copy them into the books, unless, indeed, the case was a serious one, and the temperature high. Hence the average of the temperatures in the charts is probably higher than it ought to be.

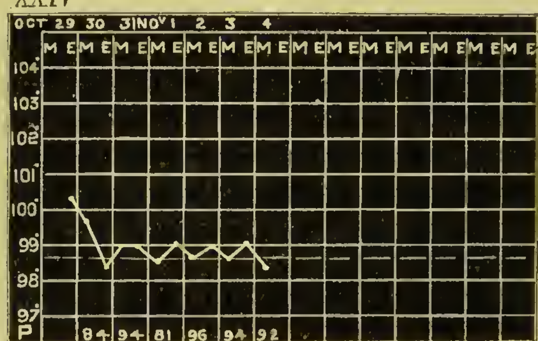
XVIII



XXII



XXIV



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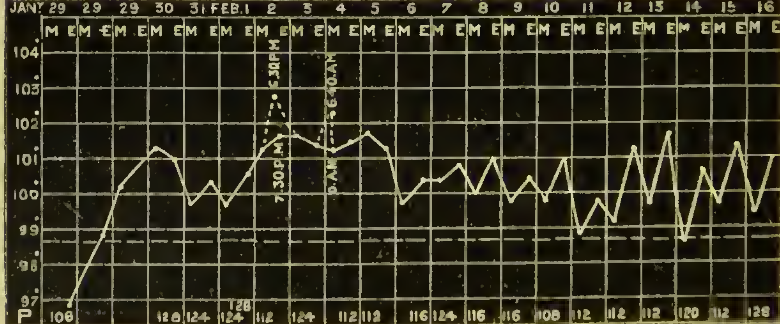
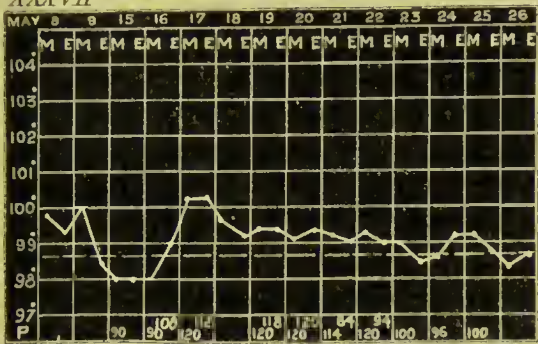
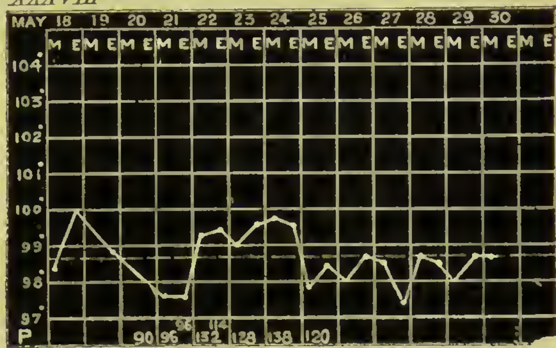


TEMPERATURE CHARTS OF SEPTIC CASES.

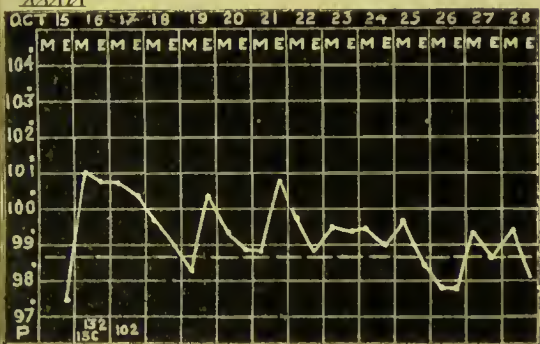
XXXVII



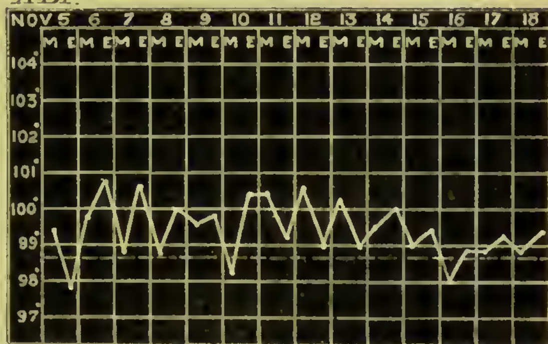
XXXVIII



XXXIX



XLI.



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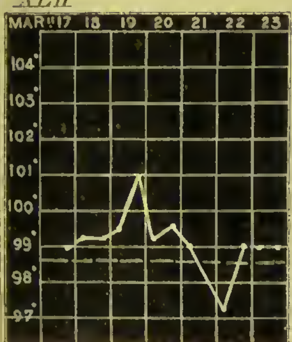

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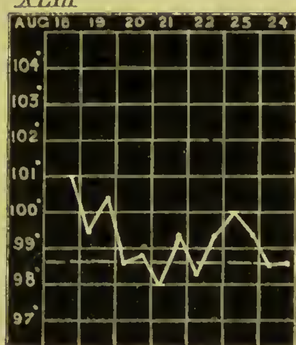

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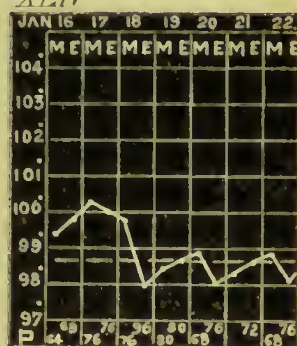
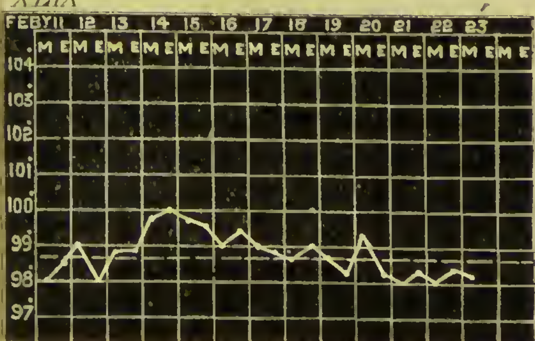


XLIX



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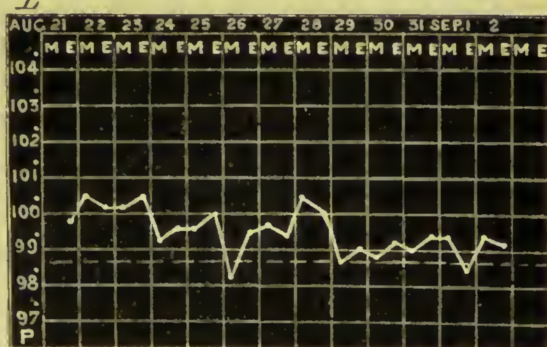

TEMPERATURE CHARTS OF ASEPTIC CASES.

and the spray and carbolic acid were employed with the view of preventing the entrance of other mischievous particles.) In this case inflammation and fever occurred. Contrast the progress of these two cases treated with antiseptics with that of the others which were treated aseptically. The same marked difference will be seen if we look at compound fractures in which fermentation was not avoided. The constitutional results in these cases have been considered in a preceding paragraph, while locally necrosis, inflammation, and suppuration are recorded in several instances. I do not say that these are the constant results in cases treated with antiseptics, but they are the results in the series of severe operations which we have been considering, and if they occur in these severe operations there is no security whatever that they will be absent from others which are less severe. That these results often follow simpler wounds, will be evident from a perusal of the foregoing chapters.

If, again, we take the open method, we find also a marked contrast between the local and constitutional consequences of wounds so treated and of those treated aseptically. Burow, as we have seen at page 334, speaks of the great swelling of the edges of the wounds, and Krönlein also tells us of the inflammation and necrosis which so frequently occur. The pain which must accompany this inflammation, and the exhaustion produced by the prolonged suppuration, are very marked features of the open method. Krönlein,[1] in his report of Rose's practice, states that it was found best not to seek primary union after amputations. He further states, that the first effect of the wound is great swelling of the flaps, which goes on increasing for some days, till, in fact, granulation is complete; this is inflammatory swelling. Krönlein also points out, that in Burow's, or any other method in which the edges of the flaps are brought together by stitches or plasters, the result of this swelling is frequently gangrene of portions of the flaps. Indeed, he says that in 58 cases of amputation treated by the method before described (no attempt being made to bring the flaps into contact) gangrene—not hospital gangrene—occurred in six instances, or in 10·3 per cent of the cases,

[1] *Loc. cit.*

simply as the result of the swelling of the flaps, combined in one or two instances with injury to them. Krönlein also states that necrosis of the end of the divided bone occurred very frequently—in 19 out of the 58 cases, *i.e.* in 32·7 per cent. This happens less often in the cases where the edges of the wound are brought together, but Krönlein tries to make out that this is chiefly because many of the patients in whom it would have occurred die before it has time to take place. This explanation is, however, to a great extent, incorrect; for the true explanation of the absence of necrosis in many cases where primary union is aimed at is that primary union has occurred over the bone, and hence the acute suppurative inflammation of the bone due to the contact of irritating materials, which results in acute necrosis, does not take place. The aseptic method, by preventing this inflammation, renders it a matter of indifference, from this point of view (the chance of necrosis), whether primary union occurs or not.

Further, in cases treated by the open method there is generally more or less fever, showing that absorption is occurring from the wound in spite of the free escape of discharge, or indicating the presence of inflammation in the wound. Krönlein does not enter into details on this subject, but he mentions the fact, that in only six, or 8·7 per cent., of the amputation wounds treated (67 in number) was there no rise of temperature; the temperature in these six cases hardly ever went above 100° F. This small proportion of cases in which there was no fever contrasts markedly with the results after aseptic operations.

The open method also possesses other disadvantages which make the contrast with the results of the aseptic method still more marked. Thus there is a constant tendency to the formation of crusts, which are apt to lead to retention of fluids and their fermentation beneath the crust; thus the wound loses the advantages of the open method. Other disadvantages have been previously alluded to.

The local results of treatment by irrigation and the water-bath are also not so good as those after aseptic treatment, and these methods possess several disadvantages. These disadvantages have been previously mentioned, and I need now only allude to one or two. Thus the water is apt to run into the

bed and wet the patient; in order to avoid this, in the case more especially of local baths, the apparatus must be so firmly fixed to the limb that swelling is apt to occur. Further, the maceration of the epidermis and the sodden state of the granulations are great inconveniences. Also where the water used is cold or where ice is too long applied there is great danger of gangrene in weak parts. Then, in the case of the water bath more especially, where the wound is deep the swelling of the granulations from imbibition of water is apt to block up the orifice and confine the discharge; and also pus coagulates in contact with the water and frequently forms a thick layer over the wound.

I need not dwell further on the points in which the other forms of antiseptic surgery are inferior to the aseptic method. Many of them are self-evident and have been already alluded to. The results, both local and general, of all methods which do not bring about an aseptic state of the wound are uncertain, and the results of these cases being uncertain the surgeon must always have a feeling of anxiety corresponding in degree to the severity of the operation and the interest which he takes in his patient.

I have previously said, that on account of the various benefits which are obtainable by the use of the aseptic method, it ought to be employed wherever possible, unless, indeed, special drawbacks attend its use. Several disadvantages, to which I must now allude, have been attributed to this method.

Firstly, it is said that there is a risk of carbolic acid poisoning. This, however, is not an objection against the *method*; it is an objection against the *antiseptic* employed, and is equally valid when a wound is treated with carbolic acid, though not aseptically. Indeed, the risk of carbolic acid poisoning is greater where wounds are not treated aseptically, for it is then employed in large quantities to wash out the wound. Where the aseptic method is carried out as formerly described, the wound not being deluged with carbolic acid either at the time of the operation or by injecting it afterwards, the risk of poisoning is very slight. The occurrence of dark coloured urine due to

absorption of carbolic acid is not very rare, but this is not poisoning. Unless a remedy is taken in such quantities that the patient's health or life are jeopardised by it, we do not say that the patient has been poisoned. In the same way, unless the absorption of carbolic acid has been so great as to produce serious symptoms, we have no right to say that the patient is suffering from carbolic acid *poisoning*.

The subject of carbolic acid poisoning has been worked out by Kuster, and Nussbaum,[1] who has had some experience of it, has written a very interesting chapter on the subject. According to them, carbolic acid kills by causing paralysis of the respiratory centre. The heart continues to beat even after the respiration is arrested, and the temperature also falls. In severe cases all the symptoms of collapse are present, low temperature, 'at first superficial and stertorous respiration, later great pallor and, finally, sudden death by arrest of respiration.' In less severe cases the following are the symptoms according to Nussbaum:—'They present at first gastric symptoms, which must really be looked on as cerebral symptoms. These are loss of appetite, frequent nausea or incessant vomiting; there is also an increase, often enormous, in the secretion of saliva, which is very frothy. The urine diminishes in quantity; indeed, it has been said that it sometimes contains albumen, but I have never observed this.[2] There may also be dysphagia, immobility of the pupil, and the patient may have a certain feeling of oppression or anxiety. The patients often lie absolutely tranquil in their bed, as if they were prohibited from moving, and they have difficulty in spitting out the saliva secreted. Fever is never absent till, in serious cases, the temperature falls just before collapse sets in.' In addition to these symptoms the urine on standing acquires a dark olive green colour, though it may have been of normal appearance when it was passed. This is due to the presence of indican.

The alleged presence of fever in cases of supposed carbolic acid poisoning is a very puzzling circumstance; for the evidence derived from experiments on the lower animals does not,

[1] *Leitfäden zur antiseptischen Wundbehandlung.* 1881.

[2] Dr. Keith stated, at the recent meeting of the International Medical Congress, that the urine in some of his cases contained albumen.

so far as I am aware, lead to the conclusion that fever is one of the results of poisoning with carbolic acid; on the contrary, it seems to be followed by depression of temperature. Under the impression that fever was one of the symptoms of carbolic acid poisoning, and wishing to produce fever in rabbits, I at one time injected carbolic acid subcutaneously at frequent intervals into a series of rabbits, but without obtaining the wished-for result. So satisfied have some physicians become of the power of lowering temperature possessed by carbolic acid that it has been employed as an antipyretic in place of salicylate of soda. These facts throw grave doubt on the view that elevation of temperature is a symptom of carbolic acid poisoning, and lead us to question whether many of the alleged cases of carbolic acid poisoning are so in reality.

Many of the cases which have been published as cases of carbolic acid poisoning are undoubtedly instances of septicæmia occurring in patients who have been treated with spray, gauze, and carbolic acid, but not by the Listerian method, *i.e.* aseptically. In these cases there is a high temperature, and it is said that carbolic acid has been found in the urine. The temperature is often distinctly septicæmic in character, while the presence of carbolic acid in the urine does not prove that these are cases of carbolic acid poisoning. On the contrary, Brieger,[1] who has investigated the subject of the formation and excretion of carbolic acid in the living body, states that carbolic acid is constantly present in the urine of septicæmic patients, often in considerable quantities, even though no carbolic acid has been brought in contact with them.

I do not wish to deny that this fever may be sometimes due to carbolic acid, but, as far as I can judge, such a view is against the evidence, though more facts are required before we can come to a decision. That there is something different in the result of administering carbolic acid internally and applying it to a wound is evident from the fact which Nussbaum mentions, that olive-coloured urine only occurs after the external use of carbolic acid, and not when it is administered by the stomach or inhaled into the lungs. Surgeons ought, however, to be

[1] 'Ueber Phenol-Ausscheidung bei Krankheiten,' *Centralblatt f. d. Med. Wissensch.* 1878, No. 30.

very careful in attributing elevation of temperature in their cases to carbolic acid poisoning.

In Mr. Lister's practice, and in that of many other surgeons who use carbolic acid freely, but who operate aseptically, carbolic acid poisoning is a thing of very rare occurrence, indeed I only know of two cases treated by Mr. Lister in which serious symptoms were present.[1] The reason of this is that Mr. Lister brings carbolic acid as little as possible in contact with wounds, but acts strictly in accordance with the aseptic principle, and *applies it freely to everything which may come in contact with the wound rather than to the wound itself.* He does not irrigate wounds, nor inject them, nor even wash away the blood and dirt from the line of incision. The surgeons who see the most numerous examples of carbolic acid poisoning are those who, led away by the dogmatic statements of eminent men to the effect that the good results of Listerism are solely due to cleanliness, apply this view to the treatment of their cases, irrigating and washing wounds freely with carbolic acid, to the great detriment of the wounds and the patients.

In the treatment of carbolic acid poisoning the first thing is of course to remove the carbolic acid. This may be done without at the same time abandoning the aseptic method. By the use of eucalyptus gauze,[2] or by the use of salicylic or iodoform dressings, the patient may have the benefit of the exclusion of organisms without the risks of poisoning. In severe cases Nussbaum advises the subcutaneous injection of three milligrammes of sulphate of atropia, which he says has a beneficial effect on the vomiting and salivation ; he also advises

[1] In both of these cases there was elevation of temperature, but it does not necessarily follow that it was due to carbolic acid. The discussion of this subject, and the speculations in which one might indulge with regard to it, are, however, not suited for the present work.

[2] I have examined, by means of Koch's method of staining bacteria, a number of wounds treated with eucalyptus gauze, and in a very considerable proportion of them bacilli were found. I therefore cannot recommend the eucalyptus dressings as being equal to those with carbolic acid. As we have already seen, micro-organisms are not found in wounds treated with carbolic acid, or, if present, they are only micrococci. I have not found bacilli under carbolic dressings. On the contrary, with eucalyptus oil, though sometimes no organisms are present, yet in a considerable number of cases bacilli may be found ; micrococci are but seldom seen.

that the patient should be packed. Where collapse is present Nussbaum has derived benefit from the use of the following means: friction of the chest, of the hands, and of the soles of the feet with a brush, and the subcutaneous injection of ether and camphor. In milder cases, besides the removal of the carbolic dressings, he advises the use of sulphate of soda as follows:—

Sulphate of Soda	5 parts.
Distilled Water	100 parts.
Syrup of Raspberries	25 parts.

Two tablespoonfuls of this mixture are given every two hours. This method of treatment was proposed by Baumann, who found that carbolic acid was not excreted by the kidneys as carbolic acid, but in the form of a non-poisonous compound with sulphuric acid. It is with the view of obtaining this non-poisonous compound that the sulphate of soda is administered. It is said not to do much good. I have myself had no experience of it.

The so-called 'carbolic eczema' has been brought forward by some writers. I have already referred to it, and mentioned the use of salicylic acid cream as a preventive. It has been attributed alternately to carbolic acid, to the paraffin and to the resin in the gauze, but as I have already explained, it seems to me in some cases more probably due to a fermentation of the discharges caused by micrococci (see p. 232).

In two cases I have known a carbolic acid dressing blister the skin in a few hours and have to be abandoned. Here, however, as I have before said, it was merely carbolic acid, not the aseptic method, which was abandoned; another antiseptic was used in its stead.

Another argument has been brought forward against the aseptic method, viz., that it distracts the attention of the surgeon from the constitutional state of his patient. Such an argument has no foundation in fact. Are the precautions necessary to attain the single object of the exclusion of micro-organisms from wounds more likely to divert the surgeon's attention from the constitutional state of his patient than the numerous cares as to ventilation, nursing, and so forth, with which the surgeon who trusts to cleanliness alone is harassed?

In the great majority of simple fractures no attention is paid to the constitutional state of the patient; the limb is placed in splints, full diet is ordered, and the patient is left till the bones unite. Why is it thought necessary to attend to the constitution of the patient when the bone is divided by an incision through the skin? Because under ordinary circumstances inflammation, suppuration, fever, and other hurtful consequences are apt to occur. But, as we have seen, the aseptic method avoids all these dangers and makes the wound practically subcutaneous. Where, then, is the necessity for attending to the constitution more than in the case of simple fracture?[1] But further, as Mr. Lister himself has pointed out, such an argument is of no weight in presence of the facts; for, if Mr. Lister gets such avowedly good results (better than those obtained by surgeons who pay great attention also to the constitution), and at the same time, as is however wrongly alleged, neglects the constitutional state of his patients, such a fact would be an additional argument in favour of aseptic treatment, and only prove the great efficiency of the method.

Then it is said that the method is costly, and therefore not applicable in the case of poor patients. Now no doubt each individual dressing is costly—costing at the most 10*d.* or 1*s.*, though generally much less—but then these dressings, after the first two or three days, require to be changed only at rare intervals, and I have calculated that in most cases, with of course some exceptions, the dressings are really in the long run cheaper than water dressing changed once or twice daily. But further, the aseptic method saves expense in many other ways. As the patient has no pain nor fever, it is only in a few cases that a trained nurse is required; any sensible friend or servant can attend him quite well. And as he is not suffering from fever nor weakened by profuse discharges, he frequently does not require stimulants or tonics, indeed in Mr. Lister's practice these are seldom ordered; this advantage is no doubt to some

[1] It must not be supposed from this that I would advise the neglect of the constitutional state of the patient. On the contrary, every care ought to be taken to attend to hygienic conditions, and by means of good diet to support his strength, or by suitable drugs to attempt to remedy any constitutional defects.

extent counteracted by the fact that the patient has an excellent appetite. Further, wounds heal on an average more quickly than when treated by the ordinary methods, and as the patient is not weakened by the presence of fever or discharge, the period of convalescence is shorter. This is, of course, of the greatest importance to the bread-winner, for he is well and back at his work while the patient who has not been treated aseptically, and whose wound has not united by first intention, is still undergoing treatment or recruiting his powers in the country. Thus in numerous ways, of which these are a few examples, expense is saved, and on the whole this treatment is from this point of view the one most applicable to poor patients.

Lastly, it has been said that the aseptic method gives the surgeon a great deal of trouble. Now there is no doubt that at first each operation and each dressing requires care and thought, but then the dressings are unfrequent, and by-and-bye the method becomes more or less instinctive. This argument of trouble could not, however, be seriously upheld for a moment, for if a system is good it must be carried out in spite of the trouble involved. Why does one take so much trouble in perfecting one's anatomical, physiological, or practical knowledge but simply with the view of being able to treat his patients better? For the same reason the trouble ought to be ungrudgingly expended here. And if there were no other reason (such as the safety and well-doing of the patient), the relief from anxiety on the part of the surgeon, and the feeling of certainty as to the result, are of themselves a sufficient reward for all the trouble bestowed. The chief point which is laid stress on as giving rise to increased trouble is the use of the spray. I have already discussed this question at pp. 73, 120, and 364, and I have also pointed out the methods by which treatment without the spray may be carried out. As I have said, the spray is the least necessary of all the precautions, because fewer micro-organisms are present in the air than on surrounding objects, and, therefore, the purification of the air is the least important: further, if any particles do fall on the wound from the air, they may be readily destroyed by washing the surface of the wound with an antiseptic lotion.

I have already pointed out that the spray can be dispensed with, and that operations can be performed without it; it cannot, however, be safely abandoned without a substitute being provided in the shape of frequent irrigation of the wound. In proof that aseptic surgery may be carried on without the spray, we have Mr. Lister's work of several years—and very successful work it was too—before he introduced the spray; but then while he performed the operation he constantly poured carbolic oil or carbolic lotion into the wound, and in dressing the case he had a current of the oil or lotion flowing over the wound. Then further, we have Mr. Callender's experience. He practically employed, with some modifications, Mr. Lister's original method with carbolic oil, and his results were good. And, in 1879, Trendelenburg [1] published the results which he had obtained without the use of the spray. He employed the method of continually irrigating the wound with carbolic lotion while he performed the operation and while he dressed the wound. And his results are, in the main, as indeed was only to be expected, aseptic results. He mentions eighteen cases where hydroceles were opened and successfully drained; eight cases where the sheaths of tendons or ganglia were opened without local reaction; twelve cases in which joints were opened, in only one of which was there inflammation; and five osteotomies, of which one died of tetanus, the others doing well. The spray, however, has advantages which seem to me far to outweigh its disadvantages. In the first place, I think it is really less trouble to have a spray playing over a part than to be continually irrigating it with carbolic lotion. Further, there is a feeling of certainty attending an operation conducted under the spray (see p. 259), for if no spray is used septic particles may fall into the wound and escape the action of the lotion; this is most likely to be the case while the wound is being stitched up, and these particles may be protected by the blood clot from the action of the carbolic acid, and may not be destroyed by the clot or the living tissues, but may develop and cause fermentation in the wound. Lastly, the spray has this great advantage over irrigation, that less carbolic acid is applied to the wound, and thus there is less

[1] *Loc. cit.*

irritation of the cut surface and less chance of absorption of carbolic acid into the system. It cannot be a good thing either for the patient or for the wound to be constantly deluging it with strong carbolic lotion.

The whole principles of wound treatment may be summed up in the one word—REST. This has been urged by many writers, from Magatus downwards, and indeed before the time of Magatus; but it is only within the last few years that science has so far advanced as to enable us to grasp the whole significance of that term as applied to the treatment of wounds. The causes of UNREST may be mechanical or chemical.[1] The *mechanical causes* consist of movement of the parts, of the presence of foreign bodies, of tension in the wound, and so on; and they are, as a rule, easily avoided by the use of suitable apparatus, by the removal of mechanical irritants, or by providing against the occurrence of tension; and inasmuch as they are easily avoided they are comparatively unimportant. The most important and the least easily prevented are the *chemical causes* of Unrest, and these may be divided into two great classes: 1. Where the chemical substance is merely something—a salt, or an acid, or an alkali—added to the wound from without, such as carbolic acid. Such a chemical cause will act only in proportion to the amount added, to its irritating property, and to the length of time that it remains in contact with the surface of the wound. When the original quantity is exhausted the

[1] Mr. Lister long ago divided the causes of suppuration into three great groups: 1. Putrefactive suppuration where it was due to the presence of putrid materials; 2. Antiseptic suppuration where it was due to the presence of some chemical substance, such as the antiseptic employed in the treatment of the wound; 3. Suppuration the result of nervous disturbance, as in tension. This classification still, I think, holds good, notwithstanding the recent work of Dr. Ogston referred to at pp. 248, 253, who has expressed the opinion that all acute abscesses are due to micro-organisms. The observations which I have published at p. 251, and others which I brought forward at the recent meeting of the International Medical Congress, seem to me to go against this view. At the Congress Mr. Lister also pointed out a number of clinical facts which proved that other causes of acute inflammation and suppuration exist besides the action of micro-organisms. I do not, of course, deny that micro-organisms are the cause of many of the acute abscesses in which they are found, but I think that in some they are accidental, and that suppuration may be induced otherwise than by their action.

chemical irritation ceases. 2. The other cause of chemical Unrest is where the chemical substance is being constantly formed in the wound. Here we have much the most formidable cause to deal with, for there is no exhaustion of the substance, but, on the contrary, continued formation of fresh material so long as the causes of this formation are present in the wound; and, as we have seen, the eradication of these causes, once they have entered, is a very difficult matter, and thus these causes of Unrest are the most important. To interfere with these causes of Unrest is the main object of antiseptic surgery. The prevention of their entrance is the special aim of aseptic treatment. The prevention of the entrance of micro-organisms is, as we have already seen, apparently much more easily and better accomplished than their destruction after they have entered. And further, in attempting their destruction after their admission, the wound is subjected in a marked degree to chemical Unrest of the first class. The ideal wound is a subcutaneous one, kept at perfect rest. We have not yet attained this ideal, for even with the aseptic method there is a certain amount of Unrest caused by the antiseptic employed, by the stitches, by the apparatus for drainage, and by the dressing itself. Nevertheless, the essential elements of Unrest have been abolished by this method, and the disturbances from the antiseptic, from the stitches, and so forth, have been reduced to a minimum, and now hardly make themselves evident. That art will still further perfect the treatment of wounds there can be no doubt; but whatever development occurs in the future, the great principles of Listerism, the exclusion of the chief causes of chemical Unrest, and the reduction of the action of the other causes to a minimum, must form the groundwork of any system.

In conclusion, I cannot too strongly express my conviction that the *scientific* basis of wound treatment should hold the most prominent place, and that it is only by a thorough knowledge of natural phenomena in all their bearings that the best practice can be carried out and the best results obtained. Loose observations and loose and vague ideas as to probabilities which have no foundation on the facts of nature, cannot advance art in any way. Natural phenomena are generally found to differ from the conception which man in his ignorance

is apt to form of them; and therefore any statements on any subject, to be of value in the development of that subject, must be founded on knowledge and rigid application of the facts of nature, whether or no these facts seemed at first sight probable or sufficient explanation of the phenomena. That advance can only be blind and imperfect till the true law of nature is discovered is well illustrated by the history of wound treatment in former years. Through the darkness which then reigned glimmers of light had at times penetrated, but no true and lasting progress was made till quite recently, when, chiefly by the scientific labours of two men—Pasteur and Lister—a flood of light has been thrown on one of the most obscure subjects in nature, and the foundation of rational methods of treatment on rational and scientific principles has been followed by inestimable advantages to mankind.

EXPLANATION OF THE PLATES.

These specimens have been drawn by the aid of the Camera Lucida, and Zeis' water-immersion or oil-immersion lenses were those chiefly employed.

PLATE I.

PLATE II.

PLATE III.

PLATE IV.

PLATE V.

Plate 1

FIG. 1

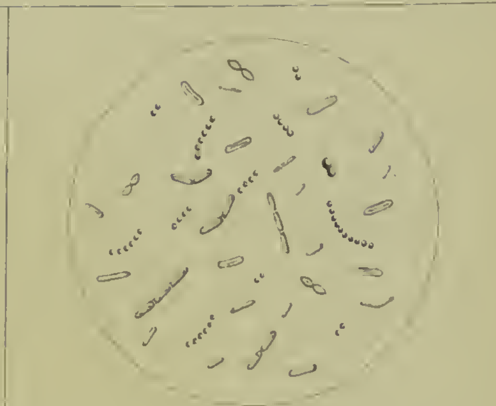

FIG. 2

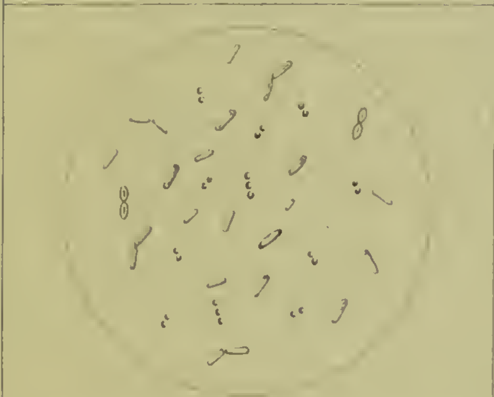

FIG. 3

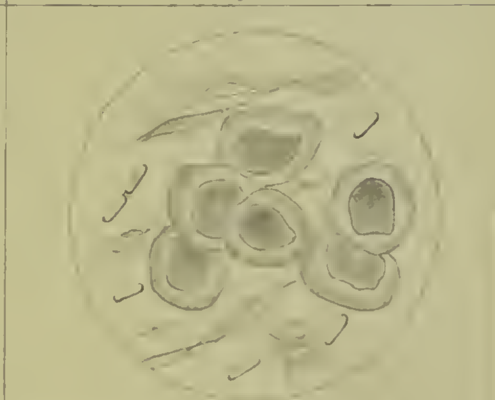

FIG. 4

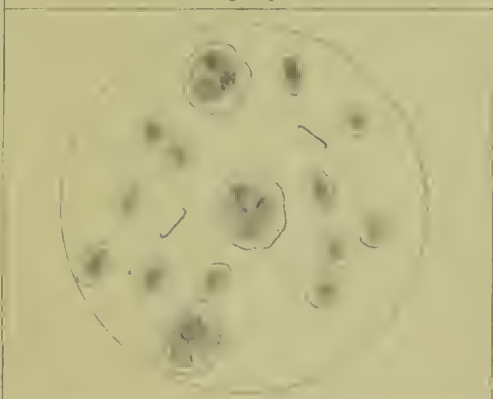

FIG. 5

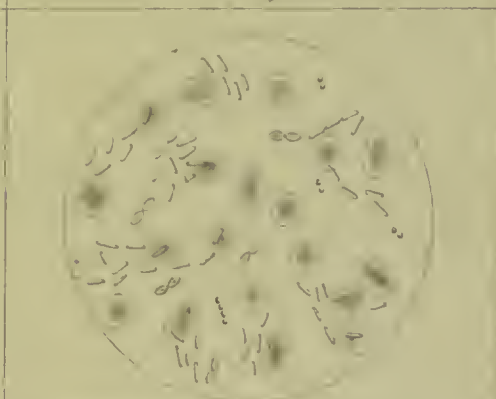

FIG. 6

FIG. 7

FIG. 8

Edgar Thurston del.

Mintern Bros. sc.

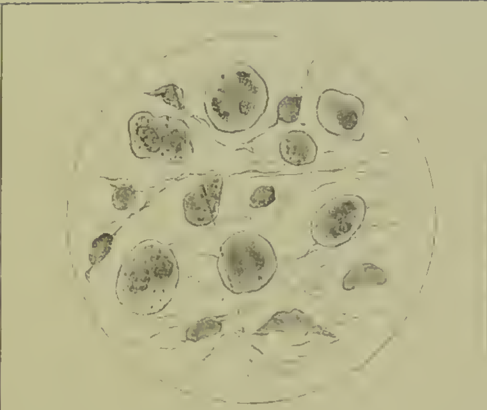

FIG. 9.

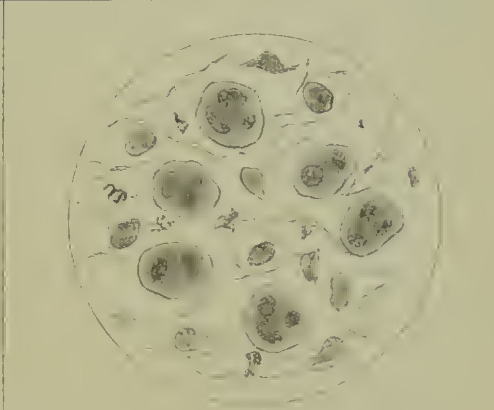

FIG. 10.

FIG.

FIG.

FIG.

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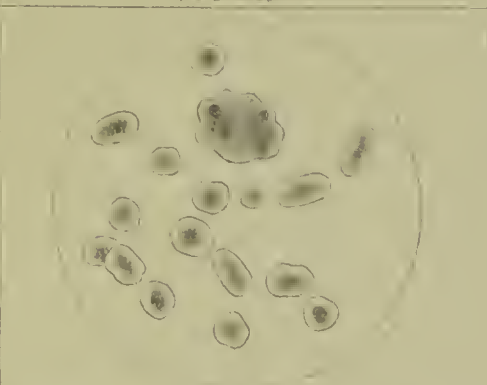

FIG. 15.

FIG. 16.

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Mintern Bros.

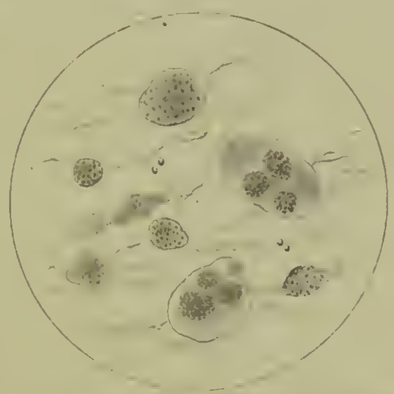


FIG. 17

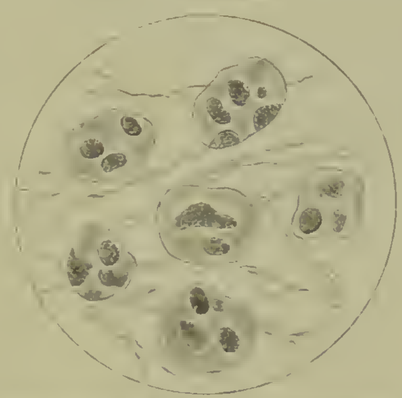


FIG. 18

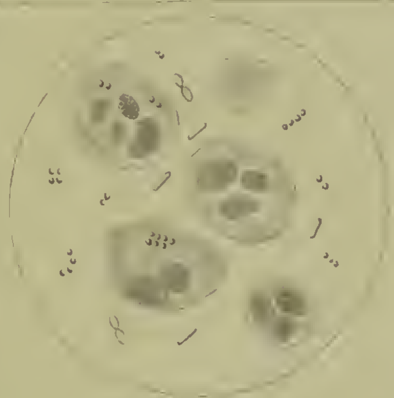


FIG. 19

FIG. 20

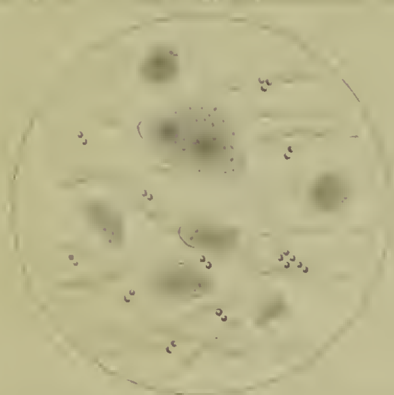


FIG. 21

FIG. 22

FIG. 23

FIG. 24

Plate IV

FIG. 25.

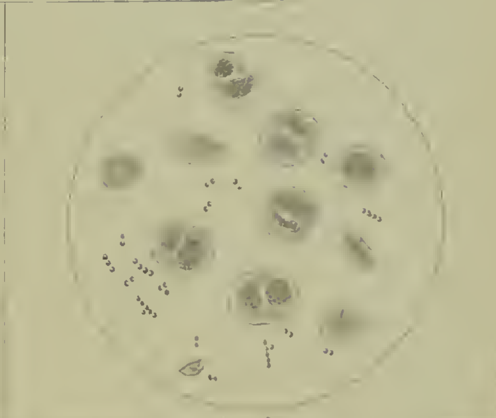


FIG. 26

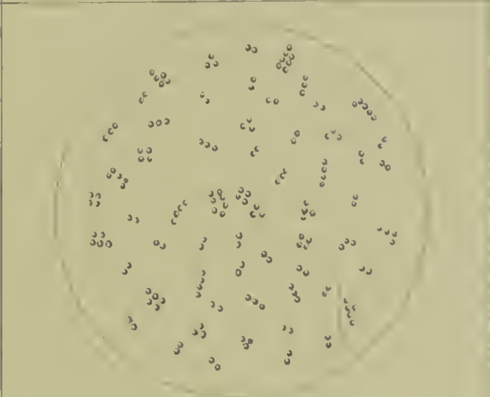


FIG. 27


FIG. 28

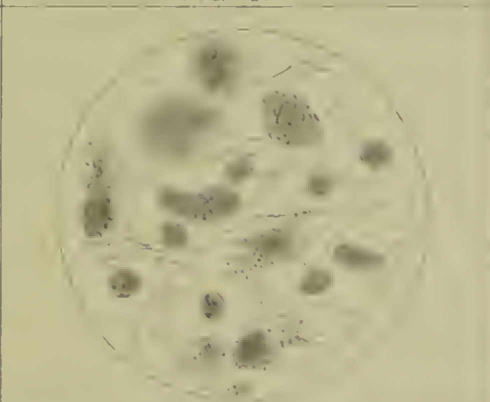


FIG. 29

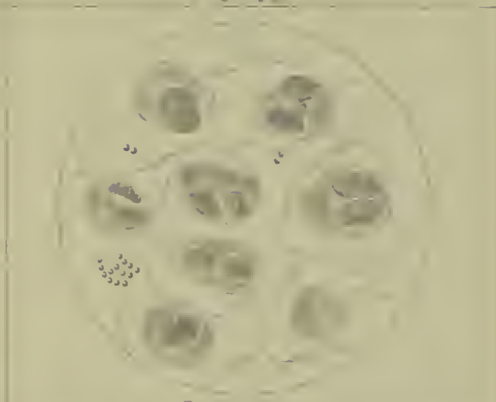


FIG. 30

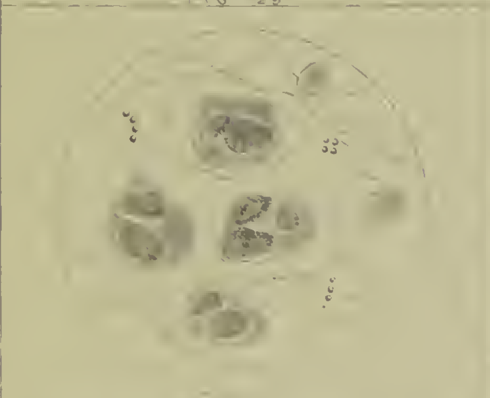


FIG. 31


FIG. 32

Plate V

FIG. 33. × 670

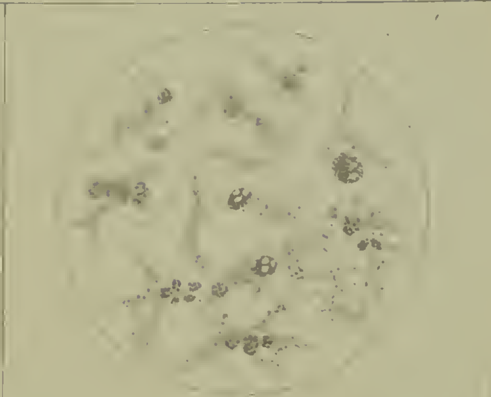


FIG. 34 × 670

FIG. 35 × 1200

FIG. [illegible] × 125

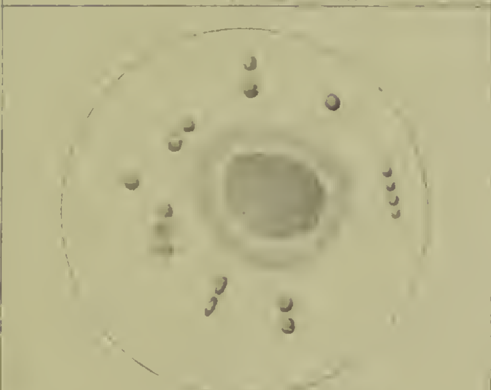


FIG. 37 × 210

[illegible] × 120

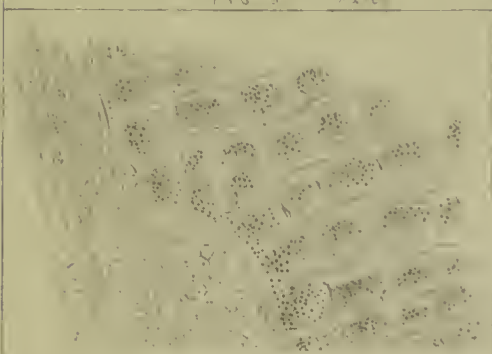
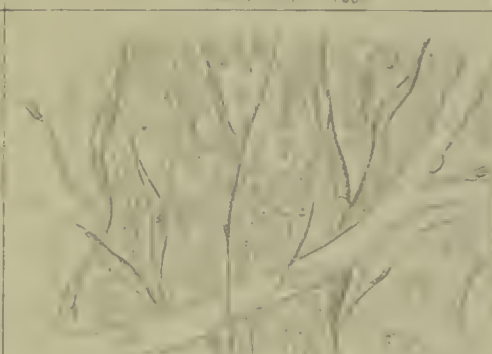


FIG. 39



[illegible]

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